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THE RELATION OF THE LABORATORY TO MEDICINE¹

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IN addressing so representative a body as this association I can not but feel that I am engaged almost in a work of supererogation when I presume to speak upon a topic so familiar to you all. Despite this feeling on my part, I am led to believe that upon occasion an oft-told tale, if palatably redressed, may prove appetizing enough: I will venture an experiment.

A careful perusal of the pages of the history of medicine plainly demonstrates the long and tortuous path pursued from the ages of earliest record down to relatively recent times. We can see how at one time the noble art was held in the clutches of superstition, its acts governed by a deep-rooted primal belief in demonology; at another time religious doctrines dominated and intimidated progress; and at other periods metaphysical discussions held back advance and even wrecked discoveries. The history of this struggle onwards towards the light of knowledge, marked as it has been by errors and lapses, is fascinatingly punctuated by epoch-making contributions here and there along the line by an occasional genius or hard-headed thinker. It is not to be doubted, however, that had it not been for the wonderful development of the sciences, the modern status of medicine would have been held back for an indefinite period. Our debt to the pioneers in chemistry, physics and biology, and to their successors down to the present moment, is enormous, and we must

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realize that instead of holding aloof from, medicine actually shares in or belongs to all of the sciences.

The development of the laboratory and its final establishment as an aid in the furtherance of medical teaching and study has been so ably presented by such an eminent authority as Professor William H. Welch in his address on the "Evolution of Modern Scientific Laboratories," that I will not attempt the foolish task of "gilding gold"; but I need only emphasize how the invaluable researches of such as Harvey, Magendie, Hunter, Bichat, Laennec, Claude Bernard, Virchow, Pasteur and Koch paved the way for the founding of institutions where ideas might be tested, problems solved and enquiring minds trained.

What a powerful stimulus, emanating from such places, has medicine received in these latter days! In fact, the laboratory constitutes the very foundation upon which medicine of to-day rests; it is the very powerful lever resting upon the fulcrum of ascertained facts that has elevated medicine from the dust of empiricism to the cloud-level of science, along which runs a road, perhaps none the less rough and long, to a goal of ultimate success.

At this point I may seem to some of you to be carried away with the greatness of the importance of the science of the laboratory at the expense of the practise of medicine. I am not. I am conscious of the great importance of both sides of medical advance and teaching. I think that we scarcely yet realize the extent of the tremendous revolution in medical thought and practise the laboratory has introduced; we are yet involved in the process, and although some of us feel that in the training of students far too much time is expended in laboratory work, profitless it may in part appear, and too little time spent in preparation for practise, I do not

doubt but that proper adjustment will come about when the evolution in progress becomes clearer to us.

Turning now to a more critical analysis of the relation of the laboratory to medicine, I will attempt to put before you in concrete fashion what I believe to be its chief functions.

In the first instance, the laboratory occupies the very fundamental position of being the place, *par excellence*, of the inductive method of impartation of knowledge; in the second, it is or ought to be the place of sound mental training and of cultivation of powers of observation; in the third, it represents applied science, and in the fourth instance it is the place of research and experiment.

Taking these up seriatim: I. Gone forever are the days of medical instruction wherein the didactic lecture played the entire rôle as the imparter of knowledge in the medical school! The lecture, will, of course, continue to hold a place in the curriculum of studies, but not so lofty or important a one as in pre-laboratory days—it has now almost assumed the humble duties of handmaid to the laboratory course, particularly in these instances where the textbook in use in a class has developed out of the yearly lectures of the head of the department.

As a place of instruction the work of the laboratory in a department requires delicate adjustment to the medical courses. I do not now propose to enter into a discussion of the relative numbers of hours to be assigned to lectures and laboratory work, nor of the time to be given to one subject of instruction relative to that of other subjects in the curriculum; at the present juncture they are irrelevant though interesting questions. Taking into consideration as granted that a laboratory is well manned and equipped, the courses should

be planned to conform to the best usage of university standards of instruction. An ample range of instruction should be afforded both in routine work, advanced work and research—all under the immediate supervision of either the head of the department or of one or other competent assistant.

In the medical courses, to properly assign and regulate the advanced work and research is sometimes a matter of no small difficulty. The number of properly qualified students, their mental caliber, their fitness for certain lines of work, the time at their disposal, are some of the many points that have to be taken into consideration in establishing and carrying on such courses. Of the two, that of advanced work is the more easily susceptible of solution. Courses may be designed throughout the year wherein work may be assigned, for a few hours a week, say five, and the nature of the topic may be so planned as to throw the student in part upon his own resources and thus encourage in him a necessary initiative. In this work the instructor can also train the student in the best ways to gain access to the literature of the subject in hand, and even demand of him a short thesis. By a properly balanced plan for advanced work we can foster a spirit for research, and perhaps gain a recruit for a task of serious investigation.

Within the limits of our ordinarily organized four-year course in medicine it seems to me to be an almost hopeless task to carry through successfully a piece of work worthy the name of research, even with our most promising students. The curriculum is so crowded and the routine so oppressive that they impose both a mental and a physical strain upon the individual, no matter how full of enthusiasm he be, that attempting research under such

conditions seems unjustifiable and indefensible.

True, research among our students should be encouraged, but let it be inaugurated at the termination of the periods of either the scientific or clinical courses, when the burden of routine may be abolished or mitigated for nine months or a year or two. If such a plan were carried out, a better choice could be made of candidates upon the basis of fitness, and would lead to the performance of investigation of a sound and creditable character, not masquerading under the name "research." By the encouragement of properly conducted research we may also be able to develop men and conserve their services at a later date for scientific pursuit and teaching instead of losing them in the alluring field of active practise. For I would have you remember that the future will make even greater demands upon us than has the past for suitable assistants and worthier successors in the fundamental branches of our profession.

II. For affording a sound mental training and for cultivating the powers of observation among a body of students, it goes without saying that the laboratory must be officered by capable persons, who, in addition to being well versed in their subject, ought to be selected also for their ability to impart knowledge. Examples can be recalled by most of us when as students we sat under men noted for their erudition but displaying an alarming innocence of even moderate pedagogical ability, which to some may have proved a stumbling-block to progress and implanted in us perhaps a veritable dislike for our studies. Or classes may be handled by instructors who are "unfaithful servants," regarding the students collectively as an intolerable burden, feeling that their duty is done if

they dispense knowledge after the manner of the "quick-lunch" counter.

On the contrary, no better reward can come to the conscientious instructor than in the very apparent, although verbally unexpressed, appreciativeness on the part of the class of his efforts to give the best that is in him. This success may be attained by giving the greatest amount of personal attention to the class as individuals, combined with general criticism, demonstration and sharp questioning concerning the occurrence of phenomena in the course of close objective study. It is quite remarkable how frequently a class of seeming mediocrity may be spurred on to good and reasonable endeavor by carefully applied methods for stimulating mental processes and awakening latent powers of observation. This leads usually to a development of a healthy independence of mind which is far removed from that mental type that is content with a senseless cramming-up of oftentimes dissociated bookish statements of facts.

Of the utmost importance is the attainment of that open manifestation in the student body of the fruits of a careful laboratory training upon entrance on the clinical period of study. For then it is that the ready aid of an alert and reasoning mind and a keen power of observation is so necessary to the student, if he is to successfully solve the thousand and one enigmas to be met daily in the dispensary classes and ward rounds. The effects of a training such as has been described are destined to last him throughout his career either as an investigator or a practising physician.

There may be some present who will be inclined to disagree with me on this statement; who already feel that our students are so crammed full of "science" that the laboratory training unfits them for acquiring a due appreciation of clinical view-

points and methods. In fact, I have heard it time and again expressed, "We don't want to turn out scientists, but practitioners of medicine!" Then, in Heaven's name, O clinicians, go to, turn our youthful so-called scientists into practitioners! The matter of so doing lies in your hands, not ours! To fail is to proclaim your own inefficiency as teachers. For I can assure you, out of an experience of some length of service, that the bulk of student material is to-day not worse than formerly, but better; more alert, more discriminating and more enquiring. He who would be their teacher must himself arise with the lark!

Another point redounding to the credit of careful laboratory instruction ought to be mentioned here. By the very processes used to develop mentally robust students, those to whom nature with niggard bounty has allotted the amount or quality of nervous gray-matter are with sureness eliminated from the race.

I maintain then that the laboratory justifies most thoroughly the high place it now occupies in the teaching of medicine, not merely from the fact that it is one of the great dispensators of knowledge, but largely because, if wisely conducted, it is the strongest of developmental forces in the successful making of future disciples of *Æsculapius*.

III. It is almost needless for me to lay emphasis on the importance of the laboratory as the place of applied science—I need only mention the daily use in hospital service of the microscope, polariscope, the X-ray, radium emanations, the many clinical, bacteriological and biological tests, and at times even the procedure of the physiologist, to prove the value of the laboratory as an indispensable adjunct to the practise of clinical medicine.

This appreciation of practical science is again shown in the establishment by city,

provincial and state authorities of laboratories more or less well-equipped for aiding the busy practitioner in his problems of service to his patients. Private laboratories for the same purpose exist in many places, where for reasonable remuneration all sorts of tests and examinations are carried out. In not a few instances men with large practises employ in their offices of consultation skillful persons, usually recent graduates, who render prompt and efficient service in clinical diagnosis.

IV. As a place of experiment and research, I feel that in the highest degree the laboratory more than justifies its existence. It constitutes the great testing-shop of ideas and theories, either generated within its walls as the result perhaps of previous experiment, or of those coming to it from beyond, the results of which may at once be made available for application in the clinical field.

As previously pointed out, medicine is no longer confined within narrow bounds, but constitutes a field of activity so wide as to demand the assistance of the other sciences to help solve its problems. And realizing full well that in this day and generation of progress in knowledge no one man is capable of becoming an expert in all of the sciences, we are in consequence witnessing medical research develop in complex form; where laboratories are established for research in physiology, pathology, chemistry, sanitary science and the like—attached to or entirely apart from the organization of medical school or university.

And so profoundly has the development of the laboratory in late years affected the course of medicine abroad, particularly in Germany, that that "holy of holies" of the clinicians, the hospital ward, is regarded as a laboratory of research, as it essentially is and properly should be. There the professor of medicine and his chief assistants

are both excellent clinicians and excellent laboratory workers; their duties to the hospital markedly limit or abolish private practise and leave ample time for carrying out instruction and research. In the United States the same development has begun; witness the establishment of the hospital in connection with the Rockefeller Institute for Medical Research, where diseases of all sorts may be intensively studied by combined clinical and scientific methods, the right of remunerative practise being denied the staff. At the present moment, too, the authorities of the Johns Hopkins University, appreciating the great value of this movement in modern medicine, are formulating plans whereby the heads of all the clinical branches be denied the right of private practise and be required to confine their whole attention to the development of ward material for purposes of instruction and research.

Reading then the signs of the times correctly, it appears that medicine has now entered upon a new and profitable era; upon a period of development wherein the scientific or laboratory idea is effecting a cleavage in the clinical field both in its methods and in its personnel. Revolutionary as it may now seem, the clinical branches in our teaching institutions in the future will probably be most largely filled by those who are at the same time competent clinicians and carefully trained workers in one or other line of scientific research, devoting most of their time to instruction and investigation and less or none to the distractions of private practise.

At this juncture I do not wish to be adjudged as one who is engaged in belittling the efforts of the clinical professor, past or present; that would be wickedly unjust, but I do believe that the time has come when not to acknowledge this evolutionary trend in clinical medicine is to

deny rational progress towards the solution of the innumerable and weighty problems confronting us.

Let me say in conclusion that it is my firm belief that in the untrammelled concentrated study of the phenomena of disease, with the ward as the laboratory, will medicine become truly scientific (in the best sense of that word), therefore truly rational, with hopes of conquest its best endeavor and success its ultimate goal. The change will evolve a man better taught, better trained and possibly possessed of better judgment. The numbers of those who practise or pursue the "art" of medicine will yet increase and reap larger and more abundant rewards in satisfaction of work well done than has hitherto been dreamed of. For the new era will demand the survival of the fittest to survive, and the practise of the profession of medicine will in even greater degree be counted the most honorable of all professions.

NORMAN MACL. HARRIS

THE UNIVERSITY OF CHICAGO

THEORIES OF SOLUTIONS¹

TWENTY-ONE years ago the Chemistry Section of the British Association at its meeting in Leeds was the scene of a great discussion on the nature of solutions. It was my first experience of a British Association meeting, and I well remember the stimulating effect of the lively discussion on all who took part in it. To-day, speaking from the honorable position of president of the section, I conceive I can do no better than indicate the position of the question at the present time. And this appears to me the more appropriate as our science has had this year to mourn the departure of van't Hoff, the founder of the

¹ Address of the president of the Chemical Section of the British Association for the Advancement of Science. Portsmouth, 1911.

modern theory of solution, whose name will remain one of the greatest in theoretical chemistry—in time to come, it will, I think, be considered almost the greatest. He had expressed the hope that he might attend this meeting as he did that twenty-one years ago. The hope is not fulfilled: his activity is merged in the final equilibrium of death. But his ideas are part and parcel of the chemical equipment of every one of us, and we know that whatever form the fundamental conceptions of chemistry may assume, the quantitative idea of osmotic pressure will be to the theory of solution what the quantitative idea of the atom is to chemical composition and properties. For I must emphasize the fact that chemistry is essentially a quantitative science, and no chemical theory, no partial chemical theory even, can be successful unless its character is quantitative. To quote the words of Lord Kelvin:

I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you can not measure it, when you can not express it in numbers, your knowledge is of a meager and unsatisfactory kind; it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of science.

A general theory of solutions must be applicable to all solutions—to those in which solvent and solute exist in practically mere intermixture, as well to those in which solute and solvent are bound together in what we can not sharply distinguish from ordinary chemical union. Between these extremes all grades of binding between solvent and solute exist, and it may be well to give a few examples illustrating the various types of solution.

Where no affinity exists between solvent and solute, the solution is practically of the same type as a mixture of two gases which are without chemical action on each other. The solute is merely diluted by the solvent

and retains its properties unchanged. An example of this type of solution may be found in the solution of one saturated hydrocarbon in another, say of pentane in hexane. On mixing the two liquids there is no evidence of union between them, the volume of the mixture is practically the sum of the volume of the components, the heat of solution is practically *nil*, the vapor pressure of each constituent is reduced merely as if by dilution with the other constituent, and so on. That there is some action between the two components even in this extreme case must be admitted, but it may be referred entirely to action of a physical kind, such as one finds on mixing one gas with another at considerable pressures. Action of a chemical nature is absent. If it be said that even saturated hydrocarbons have some chemical affinity for each other, recourse may still be had for examples to mixtures of two inactive elements, say liquid argon and liquid krypton, where chemical affinity is non-existent.

At the other extreme we have such solutions as those of sulphuric acid and water. Here there is every physical evidence of chemical union. The volume of the mixture is by no means the sum of the volumes of the components, the amount of heat evolved on mixing is very great, the separate liquids, which are practically non-conductors, yield on mixing a solution which is a good conductor, and so on. There is obviously here a great influence of the solvent water on the solute sulphuric acid, and this influence we can only account for by assuming that it is essentially chemical in character.

As the influence in such a case is necessarily reciprocal, then if even one of the constituents of the solution is inactive chemically there can plainly be no action of a chemical nature on mixing. Thus, no matter what solvent we take, it can exercise

no action other than that of a physical kind on argon, say, which has been dissolved in it; and, again, if liquid argon is chosen as solvent no substance dissolved in it can be affected by it chemically, and we thus obtain only the properties of a physical mixture. It is convenient therefore to classify liquid solvents according to their chemical activity. The saturated hydrocarbons, which are chemically very inert, and, as their name paraffin implies, little disposed to chemical action of any kind, may be taken as typically inactive solvents, analogous to liquid argon. Water, on the other hand, as its numerous compounds (hydrates) with all kinds of substances testify, may be taken as a typically active solvent. The ordinary organic solvents exhibit intermediate degrees of activity.

For the purpose of illustrating the effect of solvents on a dissolved substance one may conveniently take a colored substance in a series of colorless solvents. If the substance is unaffected by the solvent, we might reasonably expect the color of the solution to be the same as the color of the vapor of the substance at equal concentration. Iodine, for instance, gives rise to the familiar violet vapor. Its solution in carbon disulphide has a color practically similar, but its solution in alcohol or water is of a brown tint quite different from the other. In the indifferent hydrocarbons and in chloroform the color is like that in carbon disulphide, in methyl or ethyl alcohol it is brown. We conclude therefore roughly that iodine dissolved in saturated hydrocarbons, in chloroform, carbon tetrachloride and carbon disulphide is little affected by the solvent, whereas in water and the alcohols it is greatly affected, probably by way of combination, since in all the solvents two atoms of iodine seem to be associated in the molecule. That combination between the iodine and the active solvents has really

occurred receives confirmation from the behavior of iodine in dilute solution in glacial acetic acid. If the color of this solution is observed in the cold it is seen to be brown, resembling in color the aqueous solution. If the solution be now heated to the boiling-point, the color changes to pink, which may be taken to indicate that the compound of iodine and acetic acid which is stable at the ordinary temperature becomes to a large extent dissociated at 100°.

Now, as I have said, a general theory of solution must be applicable to all classes of solution, and herein lies the importance of van't Hoff's osmotic pressure theory. It applies equally to mixtures of gases, to mixtures of inert liquids, and to mixtures such as those of sulphuric acid and water; and it has the further advantage that so long as the solutions considered are dilute there are simple relations connecting the osmotic pressure with other easily measurable properties of the solutions. It has been unfortunately the custom to oppose the osmotic pressure theory of solution to the hydrate, or more generally the solvate, theory, in which combination between solute and solvent is assumed. The solvate theory is, in the first place, not a general theory, and in the second place it is perfectly compatible with the osmotic pressure theory. It is in fact with regard to a general theory of solutions on the same plane as the electrolytic dissociation theory of Arrhenius. This theory of ionization applies to a certain class of solutions, those, namely, which conduct electricity, and is a welcome and necessary adjunct in accounting for the numerical values of the osmotic pressure found in such solutions. Similarly the hydrate, or more generally the solvate, theory is applicable only to those solutions in which combination between solvent and solute occurs, and will no doubt in time afford valuable information with regard to

the osmotic pressure, especially of concentrated solutions in which the affinity between solvent and solute is most evident. It can tell us nothing about solutions in which one, or both, components is inactive, just as the electrolytic dissociation theory can tell us nothing about solutions which do not conduct electricity.

The great practical advantage bequeathed to chemists by the genius of van't Hoff is the assimilation of substances in dilute solution to substances in the gaseous state. Here all substances obey the same physical laws, and a secure basis is offered for calculation connecting measurable physical magnitudes, irrespective of the chemical nature of the substances and of the solvents in which they are dissolved, provided only that the solutions are non-electrolytes. If the solutions are electrolytes, the dissociation theory of Arrhenius, developed independently of the osmotic pressure theory of van't Hoff, gives the necessary complement, and for aqueous solutions offers a simple basis for calculation. Van't Hoff has given to science the numerically definable conception of osmotic pressure; Arrhenius has contributed the numerically definable conception of coefficient of activity of electrolytes in aqueous solution, or what is now called the degree of ionization.

Of late there has been a tendency in some thermodynamical quarters to belittle the importance of the conception of osmotic pressure. It is quite true that from the mathematical thermodynamical point of view it may be relegated to a second place, and even dispensed with altogether, for it is thermodynamically related to other magnitudes which can be substituted for it. But it may be questioned if without the conception the cultivators of the thermodynamic method would ever have arrived at the results obtained by van't Hoff through osmotic pressure. Van't Hoff was

only an amateur of thermodynamics, but the results achieved by him in that field are of lasting importance, and his work and the conception of osmotic pressure have given a great stimulus to the cultivation of thermodynamics to chemistry.

And here we trench on a question on which a certain confusion of thought often exists. To the investigator it is open to choose that one of several equivalent methods or conceptions which best suits his personal idiosyncrasy. To the teacher such a choice is not open. He must choose the method or conception which is most clearly intelligible to students, and is at the same time least likely to lead to misconception. Osmotic pressure is a conception which the chemical student of mediocre mathematical attainments can grasp, and it is not difficult to teach the general elementary theory of dilute solutions by means of it and of reversible cycles without liability to radical error or misconception. I should be sorry on the other hand to try to teach the theory of solutions to ordinary chemical students by means of any thermodynamic function. The two methods are thermodynamically equivalent, and the second is mathematically more elegant and in a way simpler, but it affords less opportunity than the first for the student to submit his methods to any practical check or test, and in nine cases out of ten would lead to error and confusion. The difficulty of the student is not the mathematical one; with the excellent teaching of mathematics now afforded to students of physics and chemistry the mathematical difficulty has practically disappeared—the difficulty lies in critically scrutinizing the conditions under which each equation used is applicable.

Of the mechanism of osmotic pressure we still know nothing, but with the practical measurement of osmotic pressure great advances have been made in recent years.

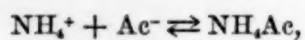
In particular the admirable work of Morse and Frazer is of the first importance in establishing for solutions up to normal concentration the relationship between osmotic pressure and composition, and its variation with the temperature. Much may be anticipated from the continuation of these accurate and valuable researches, the experimental difficulties of which are enormous.

We are indebted to America not only for these researches, and for the voluminous material of H. C. Jones and his collaborators dealing with hydrates in solution, but also to A. A. Noyes and his school for accurate experimental work and for systematic treatment of solutions on the theoretical side. They, and also van Laar, have shown how solutions not coming within the ordinary range of dilute solutions to which van't Hoff's simple law is applicable, may in some cases at least be made amenable to mathematical treatment. Van't Hoff chose one simplification of the general theory by considering only very dilute solutions, for which very simple laws hold good, just as they do for dilute gases. Even a single gas in the concentrated or compressed form diverges widely from the simple gas laws; much more then may concentrated solutions diverge from the simple osmotic pressure law. The other simplification is to consider solutions of which the components are miscible in all proportions and are without action on each other; and this method has been developed with marked success from the point of view of osmotic pressure and other colligative properties.

The outstanding practical problem in the domain of electrolytic solutions is to show why the strong electrolytes are not subservient to the same laws as govern weak electrolytes. If we apply the general mass-action law of chemistry to the electrically active and inactive parts of a dissolved substance

(the ions and un-ionized molecules) as deduced from the conductivities by the rule of Arrhenius, we find that for a binary substance a certain formula connecting concentration and ionization should be followed, a formula which we know by the name of Ostwald's dilution law. This law seems to be strictly applicable to solutions of feeble electrolytes, but to solutions of strong electrolytes it is altogether without application. Wherein lies the fundamental difference between these two classes of solutions? Two kinds of explanation may be put forward. First, the ionized proportion may not be given accurately for strong electrolytes by the rule of Arrhenius; or second, the strong electrolytes do not obey the otherwise general law of active mass, which states that the activity of a substance is proportional to its concentration. The first mode of explanation has been practically abandoned, for other methods of determining ionization give values for strong electrolytes in sufficient agreement with the values obtained by the method of Arrhenius. The other explanation is that for some reason the law of active mass is, apparently or in reality, not obeyed by some or all of the substances in a solution of a strong electrolyte. An apparent disobedience to the law of mass-action would, for example, be caused by the formation of complexes such as Na_2Cl_2 , or Na_2Cl^+ or NaCl_2^- in a solution of sodium chloride. Mere hydration, *e. g.*, the formation of a complex $\text{NaCl} \cdot 2 \text{H}_2\text{O}$, would not affect the mass-action law in dilute solution, and the electrolyte would obey the dilution law in solutions of the concentration usually considered. A somewhat similar explanation, which takes into account the properties of the solvent, is that the ionizing power of the solvent water undergoes a noticeable change when the concentration of the ions in it increases beyond a certain limit.

I should wish now to draw attention to a point of view which has not, so far as I am aware, been fully considered. To begin with we may put to ourselves the question: Is it the ions in the solution which are abnormal or is it the non-ionized substance? A simple consideration would point at once to it being the non-ionized portion. We have, for example, in acetic acid a substance which behaves normally, so that the ions H^+ and Ac^- as well as the undissociated molecule HAc are normal. Similarly in ammonium hydroxide the ions NH_4^+ and OH^- as well as the non-ionized NH_3 and NH_4OH all behave normally. When we mix the two solutions there is produced a substance, ammonium acetate, which behaves abnormally. Now, on the assumption that the equilibrium we are now dealing with is



which of these molecular species is abnormal in the relation between its concentration and its activity? Probably not the ions NH_4^+ and Ac^- , because these were found to act normally in the solutions of acetic acid and ammonia. The presumption is rather that the abnormal substance is the undissociated ammonium acetate, for this occurs only in the abnormal acetate solution, and not in the normal acetic acid and ammonia. This view, that it is the non-ionized portion of the electrolyte which exhibits abnormal behavior, and not the ions, has been reached on other grounds by Noyes and others, and I hope in what follows to deduce reasons in its support.

One is apt, because the ions are in general the active constituents of an electrolyte, to lay too much stress on their behavior in considering the equilibrium in an electrolytic solution. We are justified in attributing the fact that acetic acid is a weak acid, whilst trichloroacetic acid is a powerful one, rather to the properties of

the un-ionized substances than to the properties of the ions. The divergence of trichloroacetic acid from the simple dilution law may similarly be due to an inherent property of the un-ionized acid, a single cause being not improbably at the bottom of both the great tendency to split into ions in water and also the abnormal behavior towards dilution.

However that may be, I think the following reasoning goes far to show that the non-ionized portion of the electrolyte is that which is primarily abnormal in its behavior, the ions acting in every way as normal. The dilution formulæ of Ostwald or of van't Hoff are essentially equilibrium formulæ. One side of the equilibrium represents the interaction of the ions to form the non-ionized substance, the other side represents the splitting up of the non-ionized substance into ions. In order to fix our ideas, we may consider a salt which obeys the empirical dilution-formula of van't Hoff. If c_u represents the molar concentration of the un-ionized portion, and c_i the molar concentration of each ion, then according to van't Hoff's empirical formula,

$$\frac{c_i^3}{c_u^2} = \text{const.}$$

If the law of mass-action were obeyed we should have, on the other hand, Ostwald's dilution formula,

$$\frac{c_i^2}{c_u} = \text{const.}$$

According to this last formula, the activity of each substance concerned varies directly as its molar concentration, and a normal result is obtained on dilution. According to van't Hoff's formula as stated above, the activity of none of the substances concerned varies directly as its concentration; but since the constancy of the expression is the only test of its accuracy, there are obviously other methods of

stating the relation which will throw the abnormal behavior either on the ions or on the non-ionized substance. Thus, if we write the equivalent form

$$\sqrt{\frac{c_i^3}{c_u^2}} = \text{const.}, \text{ or } \frac{c_i^{1.5}}{c_u} = \text{const.},$$

the un-ionized substance is here represented as behaving normally, and the ions abnormally; whilst if we write the formula in the form

$$\frac{c_i^2}{c_u^{1.33}} = \text{const.},$$

the ions are represented as behaving normally, and the non-ionized substance abnormally. Now it is very important that a choice should be made amongst these three expressions, all equivalent amongst themselves so far as the mere constancy of the expression is concerned, as tested by measurements of electrolytic conductivity. Looked at from the kinetic point of view we have in the first form,

$$\begin{aligned} \frac{dx}{dt} &= kc_i^3 \\ -\frac{dx}{dt} &= k'c_u^2, \end{aligned}$$

both direct and reverse actions abnormal. In the second form, we have

$$\begin{aligned} \frac{dx}{dt} &= kc_i^{1.5} \\ -\frac{dx}{dt} &= k'c_u, \end{aligned}$$

the ionization being normal, the recombination abnormal. And in the third form we have

$$\begin{aligned} \frac{d}{dt} &= kc_i^2 \\ -\frac{dx}{dt} &= k'c_u^{1.33}, \end{aligned}$$

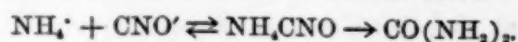
the ionization being abnormal and the recombination normal.

Now, if it were possible to measure directly the velocity of either ionization or recombination, we should at once be able to select the equilibrium formula which was

really applicable. Unfortunately such velocities are so high as to be beyond our powers of measurement. Yet it seems possible to seek and obtain an answer from reaction velocities which are measurable. One assumption must be made, but it seems to me so inherently probable that few will hesitate to make it. It is this: if a substance in a given solution has normal activity with respect to one reaction, it has normal activity with respect to all reactions in which it can take part in that given solution. Similarly, if a substance in a given solution exhibits abnormal activity with respect to one reaction, it will exhibit abnormal activity with respect to all.

Granting this assumption, we have then to find a reaction in which either the ionized or un-ionized portion of an abnormal electrolyte is converted into a third substance with measurable velocity. Such a reaction exists in the transformation of ammonium cyanate into urea in aqueous and aqueous-alcoholic solutions, which was investigated some years ago by myself and my collaborators, and found to proceed at rates which could easily be followed experimentally. First of all comes the question: Is the urea formed directly from the ions or from the un-ionized cyanate? As Wegscheider pointed out, it is impossible from reaction-velocity alone to determine which portion passes directly into urea, if the velocities of ionization and recombination are infinitely greater than that of the urea-formation, as is undoubtedly the case. Other circumstances make it highly probable that the ions are the active participants in the transformation, but we may leave the question open, and discuss the results on both assumptions.

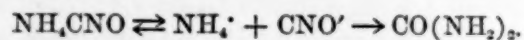
Suppose, first, that the un-ionized cyanate is transformed directly into urea. Then we have the successive reactions



The slight reverse transformation of urea into cyanate may for the present purpose be neglected, as it in no way influences the reasoning to be employed.

If the un-ionized substance behaves normally, then the conversion of the ammonium cyanate into urea, when referred to the un-ionized substance, will appear unimolecular and obey the law of mass-action: when referred to the ionized substance it will not appear to be bimolecular and will not obey the law of mass-action.

Suppose, now, that the direct formation of the urea is from the ions. Then we are dealing with the actions



Again, let us assume the un-ionized substance to be normal. Once more, if the transformation is referred to the non-ionized substance it will appear as monomolecular; when referred to the ionized substance it will not appear as bimolecular, as it should if the mass-action law were obeyed.

It is a matter of indifference, then, so far as the point with which we are dealing is concerned, whether the ionized or the non-ionized cyanate is transformed directly into urea. If the non-ionized cyanate behaves normally the action when referred to it will in either case appear to be strictly monomolecular.

If the ionized cyanate, on the other hand, behaves normally, the reaction when referred to it will be bimolecular and normal; when referred to the non-ionized cyanate it will not be monomolecular, and therefore will be abnormal.

The actual experiments show that whether water or a mixture of water and alcohol be taken as solvent, the reaction when referred to the ions is strictly bimolecular; when referred to the non-ionized substance it is not monomolecular, *i. e.*,

proportional to c_u , but rather proportional to a power of c_u other than the first, namely, $c_u^{-1.4}$.

This is, to my mind, a very strong piece of evidence that in the case of the abnormal electrolyte, ammonium cyanate, the abnormality of the ionization equilibrium is to be attributed entirely to the non-ionized portion. But ammonium cyanate differs in no respect, with regard to its electrolytic conductivity, from the hundreds of other abnormal binary electrolytes with univalent ions; and I am therefore disposed to conclude that it is to the non-ionized portion in general of these electrolytes that the abnormality is to be attributed.

As I have already indicated, this conclusion is not altogether novel, but in my opinion it has not been sufficiently emphasized. Even in discussions where it is formally admitted that the divergence from the dilution law may be due to the non-ionized portion, yet the argument is almost invariably conducted so as to throw the whole responsibility on the ions. The point which ought to be made clear is whether the constant k of the equation

$$\frac{dx}{dt} = kc_i^2,$$

or the constant k' of the reverse equation

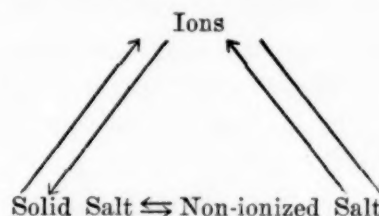
$$-\frac{dx}{dt} = k'c_u,$$

is really constant. If the former, then the ions are truly normal, and primary explanations of the abnormality of the strong electrolytes can scarcely be sought in high total ionic concentrations and the like, though a connection between the two no doubt exists, both being determined by the same cause.

In my illustration I have assumed that there holds good a dilution law of the kind given by Storch, of which van't Hoff's dilution law is a particular case. Here the active mass is represented as a power of

the concentration other than the first power. The argument I have used is altogether independent of this special assumption; the active mass of the abnormal substance may be any function of its concentration, and the same conclusion will be reached.

Nernst's principle of the constant ionic solubility product affords additional evidence that the ions act normally in solution. In deducing this principle it is generally assumed that it is the constant solubility of the non-ionized salt that determines the final equilibrium. This assumption, though convenient, is not necessary. The equilibrium is a closed one, thus:



The solid is not only in equilibrium with the non-ionized salt but also with the ions. Now, in the deduction of the change of solubility caused by the addition of a substance having one ion in common with the original electrolyte the mass-action law for ionization is assumed. This is of course justified when we deal with feeble electrolytes, but in the case of salts and strong acids which do not follow the mass-action law the experiments are found still to be in harmony with the theoretical deductions. This is not only so when the two substances in solution are both abnormal, but also when one is abnormal and the other normal, no matter which is used to produce the saturated solution. In fact, the principle of the constant ionic solubility product may be employed with equal success to calculate the effect on the solubility of one electrolyte of the addition of another electrolyte with a common ion, whether both electrolytes are normal, both abnormal, or whether one is normal and the other abnormal. At

first sight, this apparent obedience of abnormal electrolytes to the mass-action law seems strange, but a little consideration shows that if it is only the non-ionized portion of a salt that is truly abnormal, the theoretical result is to be expected. Suppose that the ions do behave normally in the ionization, then they must also act with normal active mass with reference to the solid, with which they may be regarded as in direct equilibrium according to the closed scheme referred to above. A change, then, in the concentration of any one of the ions, brought about by the addition of a foreign salt with that ion, will necessarily bring about the change in solubility of the salt calculated from the mass-action law, so far at least as experiment can tell us, for any variation from theory is caused by the change in the nature of the solvent due to the addition of the foreign substance. We ought, then, on the assumption that the ions behave normally, to expect that the principle of the constant solubility product would yield results of the same degree of accuracy in dilute solutions whether the electrolytes considered were normal or abnormal. This, as I have said, is actually the case.

To put the whole matter briefly, in the equilibrium between electrolytes agreement will be obtained between theory and experiment whether we use the mass-action law, or an empirical law such as van't Hoff's dilution formula, provided only that we attribute the abnormality to the non-ionized portion of the electrolyte. Thus we can deduce the ordinary formulæ for hydrolysis or for isohydric solutions as readily for abnormal as for normal electrolytes, and find the most satisfactory agreement with experiment in both cases.

By this one simple assumption, then, for which I have offered some direct justification, it is possible to find a basis for calcu-

lation with abnormal electrolytes. The problem of *why* certain electrolytes should be normal and others abnormal is, of course, in no way touched by this assumption. That is a matter for further investigation and research.

Another great desideratum of the theory of solutions is to find a general basis for the calculation of hydrates. The present position of the theory of hydrates in solution may perhaps most aptly be compared to the theory of electrolytic dissociation for solvents other than water. That hydrates exist in some aqueous solutions is undoubted, but no general rule or method exists for determining what the hydrates are and in what proportions they exist. Similarly the theory of electrolytic dissociation applied to other than aqueous solutions affords no general means of determining what the ions are and how great is the degree of ionization. It is only for aqueous solutions that Arrhenius was able to give a practically realizable definition of degree of ionization, and it is on this definition that the whole effective work on aqueous electrolytes is based; and until some general practically applicable principle of a similar character is attained for hydrates, the work done on that subject, however interesting and important it may be in itself, must necessarily be of an isolated character.

Arrhenius did not originate the doctrine of electrolytic dissociation or free ions: that was enunciated in 1857 by Clausius, and remained relatively barren. What he did was to introduce measurable quantities into the doctrine, and to show its simple quantitative applicability to aqueous solutions; immediately it became fertile. And as soon as a simple quantitative principle is developed for hydrates in solution, that doctrine will become fertile also.

It is surely now time that all the irrelevant and intemperate things that have

been said and written by supporters of the osmotic pressure and electrolytic dissociation theories on the one hand, and by those of the hydrate theory on the other, should be forgotten. Far from being irreconcilable, the theories are complementary, and workers may, each according to his proclivity, pursue a useful course in following either. One type of mind finds satisfaction in using a handy tool to obtain practical results; another delights only in probing the ultimate nature of the material with which he works. For the progress of science both types are necessary—the man who determines exact atomic weights as well as the man who speculates upon the nature of the atoms. That the want of knowledge as to what the exact nature and mechanism of osmotic pressure is, should prevent accurate experimental work being done on it, or interfere with its use in theoretical reasoning, is equally ridiculous with the proposition that because in the theory of osmotic pressure we have a good quantitative tool for the investigation of solutions, therefore we should abandon altogether the problem of its nature.

The fundamental ideas of a science are the gift to that science of the few great masters; the many journeymen investigators may be trusted to utilize them according to their abilities. Having once given his great principles to the world, van't Hoff remained practically a spectator of their development; but by his single act he provided generations of chemists with useful and profitable fields for their labor.

J. WALKER

THE COAL PRODUCTION OF PENNSYLVANIA

PENNSYLVANIA's coal production in 1910 was 235,006,762 short tons, valued at \$313,304,812. Of this 84,485,236 short tons was anthracite, valued at \$160,275,302, and 150,521,526 short tons was bituminous coal, valued at \$153,-

029,510. Compared with 1909, when the total production of the state amounted to 219,037,150 short tons, valued at \$279,266,824, the production in 1910 shows an increase of 15,969,612 short tons, or 7 per cent., in quantity, and of \$34,037,988, or 12.2 per cent. in value. Of the total increase 3,314,877 short tons was in the production of anthracite and 12,554,735 short tons in the production of bituminous coal. The value of the anthracite production showed an increase of \$11,093,713, or 7.4 per cent., and that of bituminous coal increased \$22,944,273, or 17.64 per cent. Although the quantity of bituminous coal produced exceeded that of anthracite by nearly 80 per cent., the value of the anthracite product was larger than that of the bituminous output by nearly \$7,250,000. Bituminous coal represented 63.6 per cent. of the total output and anthracite represented 51 per cent. of the total value.

The anthracite mines of Pennsylvania gave employment to 169,497 men, who worked an average of 229 days. The bituminous mines employed 175,403 men for an average of 238 days. The average production for each man employed in the anthracite region was 498 short tons during the year. In the bituminous mines the men averaged 825 tons each. The daily average production for each employee in the anthracite region was 2.17 short tons and in the bituminous districts it was 3.61 tons. According to the Pennsylvania Department of Mines 601 men were killed and 1,050 were injured in the anthracite mines in 1910. The fatal accidents in the bituminous mines numbered 539 and the nonfatal accidents numbered 1,142.

In the combined production of anthracite and bituminous coal Pennsylvania outranks any of the coal-producing countries of the world except Great Britain and Germany, and in 1910 it came within 10,000,000 short tons, or less than 5 per cent., of equalling the output of Germany. Pennsylvania's production in 1910 was more than four times that of Austria-Hungary in 1909, and more than five times that of France in 1910, and nearly 20 per cent. of the total coal production of the

world. From 1829 to and including the first year of the present century Pennsylvania contributed over 50 per cent. of the total coal production of the United States and still produces between 45 and 50 per cent. of the total. The industry, particularly in the bituminous districts, has kept pace with the manufacturing industries and has increased in considerably larger ratio than the population of the state and of the United States as a whole.

Anthracite mining began in Pennsylvania in 1814, when 20 long tons were produced for local consumption. The year 1820 is, however, usually considered to mark the beginning of the anthracite industry, as in that year 365 long tons were shipped from the anthracite region. From 1814 to the close of 1910 the total production of anthracite had amounted to 1,946,717,383 long tons, or 2,180,323,469 short tons.

The first records of bituminous-coal production in Pennsylvania are for the year 1840, when 464,826 short tons were mined. The total output of bituminous coal from 1840 to the close of 1910 has amounted to 2,251,737,097 short tons, from which it appears that the total production of anthracite and of bituminous coal in Pennsylvania has been nearly equal. At the close of 1908 the total production of anthracite from the earliest times to the close of that year had exceeded the total bituminous production by approximately 51,000,000 tons. As, however, the production of bituminous coal in 1909 and 1910 exceeded that of anthracite by more than 122,000,000 short tons, the total production of bituminous coal now exceeds that of anthracite.

THE MEMORIAL TO ANTON DOHRN

At a meeting of the International Zoological Congress held at Graz in August, 1910, a plan was initiated to establish a memorial to the late Professor Dohrn, the founder and director of the Zoological Station at Naples. It may be doubted whether any other single institution has equaled this one in its contributions to the progress of biology in the

past thirty years. To its development Dohrn devoted the whole energy of a singularly forceful and many-sided personality, laboring incessantly to keep the station fully abreast of modern progress, to enlarge its scope and to improve its equipment and methods, until it stood among the foremost of biological laboratories. It long since became a gathering place for investigators from many countries, and the influence that these men carried with them to their own institutions of learning made the Naples Zoological Station a potent force in the progress of biological science throughout the world.

Dohrn's far-reaching influence upon biology was due as much to his rare personal qualities as to his scientific work. He took a keen interest in the work of other investigators, even in fields far removed from his own, and was always ready with encouragement, particularly to younger men. Those who had the good fortune to come under his kindly and stimulating influence will not forget the debt they owe him. Beyond all this, the versatility of his human interests and his genius for friendship made him the center of an ever-widening circle that knew no limits of occupation or of nationality, and he was a force in the life of his time that is not to be measured by technical achievement alone but by a higher standard.

At the Zoological Congress it was proposed to establish a memorial of Dohrn's life and work, to include (1) a bronze portrait relief, to be erected in the laboratory at Naples, and (2) an endowment fund to aid in carrying on the steadily expanding work of the station. It is fortunate for the first of these aims that Dohrn had given sittings shortly before his death to the eminent sculptor Hildebrand, of Munich, who has executed a beautiful work of art that is well worthy of the present purpose. The need of additional funds for the station, as a result of the constant expansion of its work, was a subject of much concern to Dohrn in the latter part of his life. Those who knew him best feel sure that no form of memorial, could he have foreseen it, would have been

more welcome to him than the establishment of a permanent fund for this purpose.

The present movement was begun by the formation of a central committee, under the chairmanship of Professor von Graff, the president of the congress, with Professor Boveri as general secretary. This committee designated a number of persons to organize the work in various countries; and to this end national subcommittees have now been formed and are at work in most of the European countries. The American subcommittee includes about thirty biologists, and in addition a considerable number of others whose immediate interests do not lie in the field of scientific study. The hospitality and consideration which so many American students and investigators owe to Professor Dohrn, and the important influence exerted by the station on the progress of American science, justify the hope that this country will make generous response to an appeal for funds. The American subcommittee has formed an executive committee with the following membership:

Charles R. Crane, Chicago, Ill., president of the board of trustees of the Marine Biological Laboratory at Woods Hole.

Charles B. Davenport, director of the Carnegie Station for Experimental Evolution, Cold Spring Harbor, L. I., N. Y.

Frank R. Lillie, University of Chicago, director of the Marine Biological Laboratory, Woods Hole.

Jacques Loeb, Rockefeller Institute, New York, N. Y.

Hon. Seth Low, New York City.

Alfred G. Mayer, director of the Carnegie Marine Laboratory, Tortugas, Fla.

Henry F. Osborn, president of the American Museum of Natural History, New York City.

Stuart Paton, Princeton University.

George H. Parker, Harvard University.

William E. Ritter, director of the San Diego Marine Laboratory, La Jolla, Cal.

Isaac N. Seligman (treasurer), New York City.

Charles D. Walcott, secretary of the Smithsonian Institution, Washington, D. C.

Paul M. Warburg, New York City.

Edmund B. Wilson (chairman), Columbia University, New York, N. Y.

Mr. Seligman has kindly consented to serve as treasurer for the American subcommittee.

Subscriptions of any amount, however small, will be welcomed. Checks should be drawn to the order of the Anton Dohrn Memorial and sent to Mr. Isaac N. Seligman, treasurer, care of J. and W. Seligman & Co., No. 1 William St., New York, N. Y.

EDMUND B. WILSON,
Chairman of the American Subcommittee

COLUMBIA UNIVERSITY,
NEW YORK, N. Y.

SCIENTIFIC NOTES AND NEWS

MR. WALDEMAR LINDGREN, who has been connected with the U. S. Geological Survey since 1884 and since 1907 has been in charge of the investigations of metalliferous deposits and of metal statistics, has been elected chief geologist in succession to Dr. C. Willard Hayes.

It is reported that the Nobel prize for medicine will be awarded this year to Professor Allvar Gullstrand, of the Upsala University, for his work on the dioptries of the eye.

DR. CHARLES R. VAN HISE, president of the University of Wisconsin and formerly professor of zoology, has been elected a fellow of the American Academy of Arts and Sciences, of Boston. *geology*

DR. SIMON FLEXNER has received from the German government an appointment as honorary member of the Institute for Experimental Therapy at Frankfurt-on-the-Main.

THE doctorate of science has been conferred by the University of Bristol on Mr. A. P. Chattock, sometime professor of physics in the university; Professor Julius Wertheimer, B.Sc., principal of the Merchant Venturers' College and dean of the faculty of engineering in the university, and Professor Sir William Ramsay, F.R.S., sometime principal of University College, Bristol.

MR. ELI S. HAYNES, who has been in charge of the Laws Observatory at the University of Missouri, has been appointed a university fellow in astronomy at the University of California.

MR. A. B. STOUT, of the University of Wisconsin, has been appointed director of the laboratories of the New York Botanical Garden to succeed Mr. Fred. J. Seaver, who has been transferred to a curatorship.

DR. EUGENE P. HUMBERT, associate biologist of the Maine Agricultural Experiment Station, has resigned to become agronomist in the Agricultural College and Experiment Station of New Mexico.

SIR WILLIAM E. SMITH, C.B., superintendent of construction accounts and contract work, has been appointed to succeed Sir Philip Watts, K.C.B., F.R.S., as director of naval construction for Great Britain.

PROFESSOR A. J. COOK, the veteran head of the department of biology at Pomona College, Claremont, California, has been appointed by Governor Johnson horticultural commissioner of California. He succeeds Mr. J. W. Jeffrey, who has held the office for seven years. Professor Cook was for many years prior to 1891 connected with the Michigan Agricultural College. He has written a number of books about horticultural subjects and is the author of a manual of apiculture. The position to which he has just been appointed is one of great importance, especially on account of the quarantine measures permissible under the California law and which look toward the prevention of the importation of new insect pests and plant diseases.

E. R. HEDRICK, professor of mathematics; J. L. Meriam, professor of school supervision; M. F. Miller, professor of agronomy, and F. P. Spalding, professor of civil engineering, have returned to the University of Missouri after a year's leave of absence.

PROFESSOR A. E. GUENTHER, of the University of Nebraska, has been granted a leave of absence for the present academic year. He has received a special fellowship in the department of physiology of Columbia University, where he is engaged in research work.

DR. ARTHUR HOLLICK, curator of the New York Botanical Garden, has been granted a leave of absence for the purpose of continuing his study of the paleobotanical material col-

lected by him in Alaska in 1903, under the direction of the U. S. Geological Survey.

SIR FREDERICK W. MOORE, director of the Royal Botanical Gardens, Dublin, has returned from a visit to the eastern United States and Canada.

DR. CHARLES W. ELIOT, president emeritus of Harvard University, whose departure for Europe was noted in the last issue of SCIENCE, goes, it is now announced, as a representative of the Carnegie Endowment for International Peace. Dr. Eliot will proceed, *via* the Suez Canal route, to India, and after spending some weeks in that country, will reach China in the month of February. If conditions in China permit, he will make an extended journey through the interior of the country, visiting the leading statesmen and men of affairs and conferring with them as to the objects of his visit. An important part of Dr. Eliot's work will lie in Japan, which he expects to reach in April, 1912. Dr. Eliot will return to the United States in July next.

THE first course of Wagner Free Institute of Science lectures under the Richard B. Westbrook foundation will be delivered early in 1912 by Professor Morris Jastrow, Jr., on "Civilization in Ancient Babylonia and Assyria." The course will consist of five lectures, the exact dates and sub-topics of which will be announced later. The lectures will be free to the public.

AT the installation of the honorary fraternity Phi Kappa Phi at the Iowa State College of Agriculture and Mechanic Arts, on October 23, President Edwin E. Sparks, of the State College of Pennsylvania, delivered an address on "Shifting Ideals of Student Life."

DR. NANSEN, who is to give an address before the Royal Geographical Society next month, has arranged to arrive in London in time to preside at the lecture which Sir Ernest Shackleton is to deliver on Friday evening, November 3, at the University of London.

A BRONZE monument to the memory of Amedeo Avogadro was unveiled at Turin on September 24, erected, as the result of an international subscription, under the auspices

of the Royal Academy of Sciences of Turin. We learn from *Nature* that the king of Italy presided at the inauguration ceremony, which was attended by nearly all the more eminent Italian chemists and physicists, as well as by a number of representatives of foreign academies, including M. Haller, of the Paris Academy of Sciences; M. Moureu, of the Chemical Society of France; Professor Nernst, of the Chemical Society of Berlin, and M. Guye, of the Geneva Society. The date selected was the centenary of the publication of Avogadro's celebrated memoir.

GEORGE WILLIAM JONES, professor of mathematics at Cornell University from 1877 to 1907, when he became professor emeritus, died on October 29, aged seventy-four years.

M. LOUIS GRANDEAU, formerly general inspector of the French Agricultural Station, has died at the age of seventy-seven years.

PROFESSOR PAUL B. RICHTER, of the Royal Gymnasium at Quedlinburg, Saxony, who devoted much of his time to the study of the Cretaceous fossil plants of that kingdom, died on October 9, at the age of 57.

DR. JULIUS VON MICHEL, professor of ophthalmology and director of the eye clinic of the University of Berlin, has died at the age of sixty-seven.

UNIVERSITY AND EDUCATIONAL NEWS

ANNOUNCEMENT is made that the action brought by the children of the late Mrs. George Crocker to recover the residence at No. 1 East Sixty-fourth street and its contents, which form a part of the legacy given by the late George Crocker to Columbia University for the study of cancer, has been discontinued with the consent of the plaintiffs upon the payment to them by Columbia University of \$60,000, the equivalent of interest at three per cent. for two years on the amount the plaintiffs claimed.

SUBSCRIPTIONS for \$150,000 to meet the conditional pledge of \$50,000 from the General Education Board have been received by Middlebury College. One half of the fund will be reserved for general endowment, while \$50,000 will be expended for a gymnasium.

MOUNT HOLYOKE COLLEGE has received a bequest of \$5,000 from S. Newton Cutter, of Somerville, Mass., the income of which will be used for the purchase of books for the library.

MR. E. B. BURLINGAME, of Providence, has presented to Brown University his botanical herbarium of some 3,000 specimens.

BEGINNING with the session of 1911-12, the University of Missouri will require two years of college work for admission to all professional schools, except the College of Agriculture.

A NEW system of granting honors for university work has been started at the University of Wisconsin. Hereafter, special recognition will be granted at the end of the second year and at graduation. Honors at the end of the sophomore year will be granted for unusual excellence of work carried on in at least two different departments. Graduation honors will be of two kinds. First, honors will be awarded for an exceptionally original and scholarly thesis, without any consideration of the writer's previous record. Second, honors will be granted for a general high average of the required work done throughout the entire course, supplemented by independent work done in at least two subjects.

AT the annual meeting of the Association of American Universities, held at the Reynolds Club of the University of Chicago on October 26 and 27, twenty-one of the leading universities of the country were represented. The principal question before the association was the unification of the requirements for graduate work in major studies. Among those who took a prominent part in the discussion were Presidents Strong, of Kansas; Lowell, of Harvard; Wheeler, of California; Vincent, of Minnesota; Hill, of Missouri; Alderman, of Virginia, and Judson, of Chicago.

YALE, Columbia, Johns Hopkins, Virginia, Illinois and Minnesota have joined in an arrangement for an exchange of professors with Japan. Under the terms of this agreement Japan will be represented for four weeks at each of the above named institutions, the coming year by Dr. Ignazo Nitobe, of Tokio,

Japan. Each of the seven universities contributes five hundred dollars every other year to send a representative from the United States to Japanese universities.

PREPARATIONS are under way for the centennial commencement of Hamilton College on June 17, 1912. Senator Elihu Root, chairman of the board of trustees, has announced that President Taft and Vice-President Sherman will deliver addresses.

GOVERNOR STUBBS, of Kansas, Chancellor Frank Strong and regents William Allen White, Rodney A. Elward and Scott Hopkins, of the University of Kansas, have spent three days at the University of Wisconsin studying its methods with special reference to the extension of its work in education throughout the state.

DR. THOMAS E. HODGES was installed as president of the University of West Virginia on November 1.

By the appointment of Professor H. C. Peffer as professor of chemical engineering at Purdue University, this department has been raised to the status of a school coordinate with those of civil, electrical and mechanical engineering, and made independent of the department of chemistry, which, however, will continue to give instruction in general, organic and analytic chemistry. Professor Peffer will, during the current year, give instruction to seniors in industrial organic chemistry and metallurgy, and direct the preparation of graduation theses. Professor Peffer is a graduate (B.S. 1895 and M.S. 1907) of Pennsylvania State College and has been connected as chemist or superintendent with the Carnegie Steel Co., the Pennsylvania Salt Co. and the Pittsburgh Reduction Co., now the Aluminum Company of America.

At the University of Missouri the following appointments have recently been made: W. J. Calvert, M.D. (Johns Hopkins), professor of preventive medicine; J. A. Ferguson, M.F. (Yale), professor of forestry; R. H. Baker, Ph.D. (Pittsburgh), assistant professor of astronomy and director of the Laws Observatory; H. L. Kempster, B.S. (Michi-

gan Agricultural College), assistant professor of poultry husbandry; Lawrence G. Lowrey, A.M. (Missouri), acting assistant professor of anatomy; A. J. Meyer (formerly of Wisconsin), assistant professor and superintendent of the two-year course in agriculture; Matthew Steel, Ph.D. (Columbia), assistant professor of physiological chemistry; G. S. Dodds, Ph.D. (Pennsylvania), instructor in zoology; O. F. Field (formerly of Nebraska), instructor in physical education; R. L. Gainey, A.M. (Washington University), instructor in botany; Paul Phillips, B.S. (Missouri), instructor in manual arts; Ralph E. Root, Ph.D. (Chicago), instructor in mathematics; W. A. Tarr, S.B. (Arizona), instructor in geology and mineralogy. The following promotions have been made: E. A. Trowbridge, from assistant professor to professor of animal husbandry; C. B. Hutchinson, from instructor to assistant professor of agronomy; Horace F. Major, from instructor to assistant professor of landscape gardening; O. W. H. Mitchell, from instructor to assistant professor of pathology; H. C. Rentschler, from instructor to assistant professor of physics; J. C. Hackleman, from assistant to instructor in agronomy; L. G. Rinkle, from assistant to instructor in dairy husbandry; Warren Roberts, from assistant to instructor in civil engineering.

DISCUSSION AND CORRESPONDENCE

CHROMOSOMES AND ASSOCIATIVE INHERITANCE

THE difficulties that Emerson finds in the chiasma type hypothesis are not, I think, as serious as he states (*SCIENCE*, October 20, 1911); and since the hypothesis appeared to meet the situation so exactly I ventured to suggest that it might be worth consideration. My brief reference to this postulated mechanism (*SCIENCE*, September 21, 1911) seems not to have been entirely understood by Emerson, for which the brevity of the statement, or failure to express myself clearly may be responsible, but by reference to Janssens's paper ("La Cellule," 1909) I had hoped a brief statement would suffice. In fact, the only difficulty of any weight raised by Emerson is not a dif-

difficulty at all when the chiasma type of cross union between the homologous chromosomes is grasped; the difficulty arising rather from my attempt to express in a sentence or two the essence of the mechanism described by Janssens. I said that the well-known twisting of the chromosomes giving a spiral line of separation was followed by a splitting in a single place. Emerson properly objects that if this were strictly carried out some of the genes, those at the nodal points, might be divided quantitatively. In reality according to Janssens the chromosomes break at the nodal point and unite so that the two resulting chromosomes consist of pieces (two or more) of each of the original members of the pair. If, then, the genes do not themselves split when the chiasma is formed there is no opportunity offered for a quantitative division. This is the mechanism that Janssens describes as I understand it.

A second point raised by Emerson is likewise not a serious difficulty, although we need further facts in different animals and plants concerning the nature of the chiasma type before we can speak positively about the matter. Emerson asks how if the mechanism explains the facts of coupling in those cases where some interchange must be admitted (in a case like that of *Drosophila*, for example), can we account for the purity of certain races where certain characters remain coupled and never interchange? My answer is, first, that the hypothesis was offered primarily to account for those cases where the coupling is not absolute and crossing must be admitted; and second, that whether interchange does or does not occur will depend primarily on the nature of the chiasma type; whether, for example, crossing takes place at certain levels (stations) more likely than at others or whether it is entirely a chance crossing. Until cytologists have settled this matter we may leave the question open; but the very latitude that this mechanism offers seems to me to fit the situation far better than one that admits of no such freedom; for the facts themselves are diverse. That complete coupling of several characters may exist, such as

yellow, black and chocolate in mice is clear; and the result in such cases may be due to the region of the chromosome (that contains the factors for these colors) holding together as a unit when the chiasma forms; while in other cases the union between a similar series of factors may not be so close, so that crossing is more likely to take place.

As to "what has become of the 'individuality' of the chromosomes" if interchange between homologous pairs be admitted, is a matter of very small consequence; since Boveri, who is the chief exponent of the hypothesis of individuality, has long since admitted such an interchange in his definition; and since the facts of Mendelian inheritance call for such an interchange, if the chromosomes be admitted as the most likely vehicles of hereditary factors. All that my hypothesis pretends to account for is that groups of factors that enter together tend to remain together. The chiasma type appears to explain how such union may remain; perhaps some other mechanism may be found that will do as well. *The important point is that the coupling (association) of sex-limited characters that I have found in Drosophila shows that the factors must be referred to the same chromosome, and if so there seems to be no escape from the conclusion that interchange as well as association must be admitted on the chromosome hypothesis.*

Emerson has himself suggested a view to explain the remarkable cases of coupling that he has found in corn.¹ His hypothesis requires that in those cases where no interchange takes place the coupled factors lie in homologous chromosomes, while in those cases where interchange takes place the same or similar factors are contained in non-homologous chromosomes. This may seem probable or improbable, as one prefers, but in the case of *Drosophila*, where the factors in question are sex-limited and coupled with the sex chromosome, we see that his hypothesis can not hold, and that the facts can be explained without need of such an hypothesis.

¹ Annual Report Nebraska Agricultural Experiment Station, 1911.

If I might venture to point out what seems to me to be the weak point in my own view I should regard the evidence that the crossing observed in the chiasma type really takes place is by no means as yet established (see Gregoire, "La Cellule," 1910); for, while the twisting can not be doubted it is still an open question as to whether the chromosomes may untwist before the "split in one plane" appears.

T. H. MORGAN

COLUMBIA UNIVERSITY

THE COTTON WORM

TO THE EDITOR OF SCIENCE: In connection with the correspondence of Dr. H. T. Fernald in the October 13 issue of SCIENCE on the cotton worm in Massachusetts, it may be interesting to note that there has been a very heavy migration of this insect (*Alabama argillacea* Hubn.) in the city of Pittsburgh this year. The moths began to arrive about the tenth of September and reached the maximum numbers on September 23, on which date hundreds were to be found on electric light poles and buildings in the heart of the city and passing street cars stirred up swarms from sunny places. The insects are still present (October 17) but not in very large numbers.

JOHN L. RANDALL

THE AIR BLADDER IN *CLUPEA HARANGUS*

IN SCIENCE (October 13, 1911) I described the air-bladder of *Ophiocephalus* and called attention to the desirability of an investigation of the condition of the posterior duct to the air-bladder in *Clupea harangus*. In this connection Dr. Gill has kindly called my attention to a lecture by Professor Huxley, published in *Nature* (April 28, 1881) in which he (Huxley) shows conclusively that *Clupea* has the posterior duct actually open to the exterior.

E. C. S.

QUOTATIONS

BENZOATE OF SODA AGAIN

THE American public believes that a question is not settled until it is settled right.

This probably accounts for the fact that the sodium benzoate question will not down. And yet, although volumes have been written on this much controverted subject, the problem itself is really a simple one. There are three basic facts on which all are agreed: First, no one denies that sodium benzoate in foods may prove harmful in certain quantities, under certain conditions or when given to certain classes of individuals. Second, no one denies that foodstuffs of a high quality can be put up without the use of sodium benzoate; in fact the best food manufacturers do not use this chemical. Third, no one denies that when this chemical is used, scrupulous cleanliness and extreme care in handling are no longer necessary. These are three incontrovertible facts, admitted grudgingly or frankly, as the case may be, by both pro- and anti-benzoate forces. Under the circumstances, then, it is not irrational to conclude that sodium benzoate should not be used as a food preservative.

And now comes from Berlin the "Expert Opinion of the Royal Scientific Deputation for Medical Affairs Regarding the Use of Benzoic Acid and its Salts for the Preservation of Food." These experts were requested by the Minister of Education and Medical Affairs in Germany to give their opinion on this subject. In their report, they first describe the chemical and physiologic action of these drugs and then briefly summarize the findings of various scientists on the question at issue. Of the decision of the United States referee board, these German scientists say:

The series of experiments in this connection made by the American scientists are of too short duration and the results coupled with certain limitations, so that they can not be regarded as demonstrating the unconditional non-injurious nature.

After considering all of the evidence on the subject the Scientific Deputation for Medical Affairs reaches the following conclusions:

In regard to the admissibility of the use of benzoic acid and its salts for the preservation of food it is mentioned that in France on the basis of a decision of the Comité consultatif d'hygiène publique of October 1, 1888, the Minister of Justice

in his circular dated October 16, 1888, prohibited the use of benzoic acid in drinks and food.

In Austria the Supreme Sanitary Council in an expert opinion dated December 16, 1899, decided in favor of a prohibition of preserving substance containing benzoic acid or its salts, and has adhered to this standpoint in a recent expert opinion and given a detailed justification of the same. In the same sense the Saxon Landes-Medizinal-Kollegium expresses itself.

The Scientific Deputation for Medical Affairs is likewise of the opinion that the use of benzoic acid and benzoic acid salts for the preservation of food should not be permitted. Even if small doses of the same may be considered harmless for the human organism there is still a danger that, with the addition of these substances to the various food and drinks on the whole quantities would be daily consumed, which would be injurious to the organism. This fear is particularly justified in the case of children, the aged, and weak or sick persons, whereby it is to be observed that even in the case of normal food not preserved with benzoic acid substances are introduced from which benzoic acid comes into existence in the body.

A further objection against the use of chemical preservatives at all consists in the fact that in its use the food intended for consumption may not be handled with the necessary care and cleanliness to prevent its decay or injury by fungi and that, being neglected by the manufacturers and sellers, under certain circumstances the quality of the goods would suffer. . . .

Similar objections exist regarding albuminous food liable to decay. The experiments of the imperial health office have demonstrated among other things that a slight smell of decay in chopped meat may be concealed, but not entirely removed, by merely stirring or turning over the meat; but on mixing with 0.25 per cent. benzoic acid or sodium benzoate the smell disappears for a time. By this process, therefore, food which has already commenced to decay can be given the appearance of freshness and the purchaser deceived as to its quality.

These findings agree exactly not only with the opinion officially expressed by the American Medical Association in its resolutions on this subject, but also with the opinion held by other scientific bodies and by the intelligent public generally. With the reorganization of the Department of Agriculture, which is as inevitable as it is necessary, it is to be hoped

that the United States government will soon cease to hold its present inconsistent position on the subject of the use of sodium benzoate in foods. This chemical has no place in the dietary of any people and certainly its legalized use is a disgrace to an enlightened nation.—*Journal of the American Medical Association*.

SCIENTIFIC BOOKS

Memorial Volume Commemorative of the Life and Work of Charles Benjamin Dudley, Ph.D., Late President of the International Association for Testing Materials and of the American Society for Testing Materials. Published by the American Society for Testing Materials, Philadelphia, Pa. 1911.

The book is, in fact, the proceedings of a memorial session held by the American Society for Testing Materials on June 29, 1910. The proceedings began with the presentation of a sonnet in memory of Charles Benjamin Dudley by Harvey W. Wiley and closed with a personal tribute by Robert W. Hunt. Other contributions to the proceedings consisted of discussions of several phases of Dr. Dudley's character, his life, and his work, by the different officers and members of the association, respectively, as follows: Introduction, by Vice-president Robert W. Lesley; Dr. Dudley as a Railroad Man, by Theodore N. Ely; Dr. Dudley as a Chemist, by Edgar F. Smith; Dr. Dudley as a Metallurgist, by Henry M. Howe; Dr. Dudley as a Mentor, by B. W. Dunn; Dr. Dudley as a Citizen, by W. H. Schwartz. These discussions were followed by minutes and announcements on the death of Dr. Dudley and copies of various papers and addresses by him.

The discussions of the phases of his character and life were all highly eulogistic, as might be expected, but everything said was fully justified. His life and character were worthy to be studied and copied by all, and particularly to be studied and used as an example and inspiration for young men. Mr. Lesley well summed up his character when he said, "he was a diplomat of the heart, a nobleman of nature's handiwork . . ." and further

he said most justly, "he was a kind and generous friend to the young men and particularly solicitous for their advancement."

These were the qualities which made Dr. Dudley so successful as president of the American Chemical Society. In that high office his kindly diplomacy and great tact enabled him to harmonize many conflicting interests, and to so largely help to advance the interests of the society and bring it to that excellent condition of harmony and efficiency which now prevails.

This book will be a valuable addition to all libraries and particularly to those of the younger generation. It should be read and pondered by all men.

WILLIAM MCMURTRIE

Taschenbuch für Mathematiker und Physiker.

By FELIX AUERBACH and RUDOLF ROTH. Leipzig, B. G. Teubner. 2 Jahrgang, 1911.

The second volume of the "Taschenbuch" consisting of 580 pages, may not correspond to the American idea of a "Taschenbuch," but it is an unusually convenient "Handbuch" for mathematicians and physicists. A part of the table of contents is of value only or chiefly to residents of Germany—the calendar for Berlin, the table of magnetic elements for central Europe, the "Verzeichnis der Hochschullehrer"—but with these exceptions the entire book is of general interest. The articles dealing with astronomical facts concerning planets and comets, the tables of astronomical and geodetic constants, the four-place logarithm tables of numbers and trigonometric functions, the tables of squares and Bessel functions, the numerous tables of all the important physical constants, call for no review. One notes, however, how admirable is the synopsis of the fundamental definition and operations of mathematics. A candidate for a doctor's degree in physics would do well to master the mathematical portion of this volume. Not only is here given the theoretical groundwork of the subject, there are also given labor-saving applications; *e. g.*, the complete Fourier's series are worked out for a number of common

forms of the function. There is also an application to life-insurance mathematics.

The synopsis of the fundamental principles of physics, while lacking the continuity of the mathematical synopsis, is none the less complete. There is here condensed what one ordinarily finds spread over several volumes of general physics.

The article which will be of the greatest interest to readers of the "Taschenbuch" is that on the principle of relativity by Willy Wien. It is an historical and a critical summary, complete at least in its physical aspect. The contributions to this theory made by Minkowski are briefly set forth not only in this article but also in the review of Minkowski's work with which the book opens. That one who has contributed so much to this far-reaching theory should be cut off in the very prime of his power is to be greatly deplored. Physicists and mathematicians will be pleased to have the portrait of Minkowski which accompanies the article. G. F. HULL

SPECIAL ARTICLES

CONCERNING A NEW ARRANGEMENT OF THE ELEMENTS ON A HELIX, AND THE RELATIONSHIPS WHICH MAY BE USEFULLY EXPRESSED THEREON

In this abstract of a paper which, under the title "Helix Chemica," has been published in *The American Chemical Journal*, Vol. XLV., p. 160, 1911, the writer wishes to explain briefly the grounds of the proposed arrangement and to illustrate by a few examples the many uses to which the helix may be put to bring out and compare the complex relationships of the elements.

In Fig. 1 the helix is presented from the side, in Fig. 2 from the end, where of course the front curve of each series hides those behind it. In Figs. 3-6 the curves are drawn as if they were on the end of a barrel, enabling one to see the groups and series at the same time. A great number of harmonic relations are presented on these figures, only a few of which can be discussed in this abstract. The system uses the series of Mendeléeff, but makes one half of each group the antithesis,

instead of the associate, of the other, thus separating dissimilar elements like copper and potassium.

The elements are placed in the order of their atomic weights, and with interspaces equal to the average difference of the atomic weights of adjacent elements. These differences are found to increase for the different groups of rings in the simplest arithmetical ratio 0 1 2 3 4, where, since the atomic weights are rarely whole numbers, the ratio numbers will not be exact digits, and the first term will be some very small number instead of exactly zero. The regularly recurring resemblances of the elements break them up into groups determining the number of circles of a given size and the number of interspaces and of elements upon each kind of circle; and all these relations are according to the same simplest geometrical ratio, 2 4 8 16 32. The length of each circle is obtained by combining these ratios.

Starting with the best determined circles, the lithium ring covers a range of atomic weights equal to sixteen ($H - Ne = 16$) and contains eight elements (an octant), and so the average interspace is put as two. The sodium series is wholly symmetrical with the Li ring, and so is put on a second equal coil of a spiral beside it. The third ring agrees exactly with the other two from potassium to titanium, potassium having valence and sp. gr. about like sodium and so on to titanium, which has valence and sp. gr. about 4, like silicon. Here the resemblance ceases, and vanadium, instead of agreeing with phosphorus, continues downward with valence and sp. gr. 5, and so on to the iron triad group with valence and sp. gr. equal to 8; and then the curve turns; valence and sp. gr. grow less (copper = 7, zinc = 6, etc.), until the last half of this band from Ge to Br is exactly homologous with the rearward half (in Fig. 1) of the preceding circles. Thus is established a second larger type of circle containing sixteen elements (a double octant), and making an advance in atomic weight of about 48, so that the interweight becomes about 3 and the interspace in this circle is made 3. This changes

the spiral into a helix. Four such circles can be constructed. If there are two octant circles with an interspace of 2, and four double octants with interspace 3, there should be 8 quadruple octants with interspace 4, but the curve advances only one quadrant of the first circle of this type, and does this with an interspace of 4, but becomes so complex that it falls asunder spontaneously, giving up atoms of helium whose combining weight is 4, indicating that the additions in this last curve have been by fours.

In the other direction symmetry demands a diminishing of the helix to a single half octant with interspace 1, and hydrogen stands at the beginning and helium (4) at the end of this ring. One must search among the nebulae for elements light enough to fill the two gaps, and we find among the simpler spectra in the simplest nebulae the lines 4,340 t.m. and 4,862 t.m. belonging to hydrogen, the next higher line, 4,959 t.m., belonging to nebulium, which has probably greater density than hydrogen because it is found more concentrated in the center of the nebulae, for which reasons we may assume nebulium to be a dyad and to take the second place in our circle. The next higher line, 5,007 t.m., I have tentatively assigned to the next element in this half-octant, which, from its position, must be a halogen, and which I have called proto-fluorine. The next higher number, 5,876 t.m., belongs to helium—which completes the circle.

The helix must close with the half of a quarter-octant—a single element which must have valence = 0, a density much less than hydrogen, and atomic weight much less than unity. Where would one search for such an element better than in the corona? Indeed, coronium is found to be probably lighter than hydrogen, since its lines are found further from the sun than those of hydrogen. It occupies also the position of the second element extrapolated by Mendeléeff for which he obtained the atomic weight 0.4, and suggested its identity with coronium. I have, by a similar method, obtained the value 0.3, and the symmetry of the helix would suggest that it should be still smaller.

There remains only the origin of the curve where I have placed the letter *E* for the ether, electron, protyle or Urstoff, which must have a valence and density equal to zero,¹ and we may also almost say with an atomic weight of zero, since *E* is the *x* of Mendeléeff for which he calculated a hypothetical atomic weight equal to one-millionth that of the hydrogen atom. The numerical relations of the helix are summarized in the following table. The positions and the number of the elements on the different circles may be obtained by drawing a regularly increasing number of diameters to each succeeding type or circle. One vertical diameter is drawn in the quarter-octant, giving place at its ends for two elements. Two diameters are drawn at right angles in the half-octant for four elements. The angles are bisected by two other diameters, giving place for eight elements in the octants; and then again bisected, giving sixteen for the double, and a final bi-

TABLE OF THE SYMMETRIES OF THE HELIX

	Quarter Octave	Half Octave	Octaves	Double Octaves	Quadruple Octaves	
1. Length of inter-space (= inter-weights) in each type of circle. (Approximate) Unit-curve = $C_n - H$.	0	1	2	3	4	Simplest arithmetical ratio.
2. Number of inter-spaces in each type of circle.	2	4	8	16	32	Simplest geometrical ratio.
3. Number of elements in each type of circle.	2	4	8	16	32	
4. Number of circles of each type.	$\frac{1}{2}$	1	2	4	8	
5. Number of elements in each circle, obtained from 4 and 2.	1	4	8	16	32	Combination of above.
6. Length of each type of circle, obtained by multiplying preceding ratios in 1 and 2.	0	4	16	48	128	

¹ G. T. Stoney, "The Non-existence of Density in the Elemental Ether," *Phil. Mag.* (5), Vol. 29, p. 467.

section gives thirty-two for the quadruple octants.

The helix is placed horizontally, so that the nullivalent elements shall form its axis—they being the lightest elements—from which each group is continued downward with symmetrical increase in valence and density on either side to a maximum in the triads at the bottom. The alkalis on the one side are the antitheses of the halogens on the other, and this maximum of dissimilarity decreases downward symmetrically on either side. In the double octants it reaches a valence of 8 and a corresponding specific gravity. In the octants the circle closes with a valence of 4. It is interesting that carbon, the element of life, and silicon, the element of the rocks, form the center of the figure and are both trimorphic like the triads at the bottom of the double octants. If the helix be cut along its axis and the curves opened out on a flat surface the table of the elements given below results. This shows that the newcomers intercalated between the homologues of the preceding group are three for the octave, nine for the double-octave and seventeen for the quadruple octave if there be a triad at the bottom of the large curve. Symmetry would demand a group of twelve at that place.

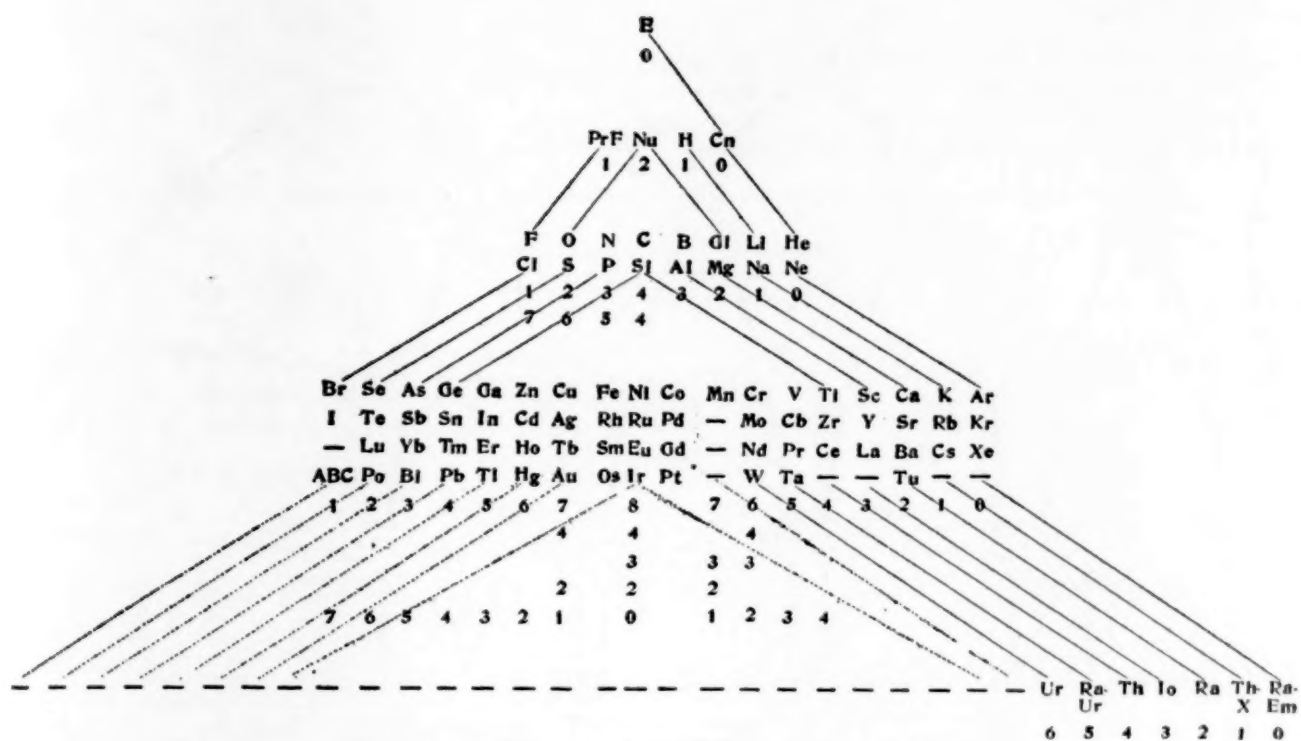
If the helix be cut along the lower line of densest elements and flattened out we have the following table, where, as the lowest elements have an equal right to be placed at either side, they are placed on both sides. This suggests the possibility that the second element beyond uranium would be the first member of a triad.

Doubtless a better suggestion would come from the preceding table, that there would be a triad or larger group at the bottom of the larger circle.

I. Longitudinal Relations. Fig. 1.

At the bottom of Fig. 1 is placed the atomic volume graph of Meyer: (1/sp. gr.) the specific volume graph of A. J. Hopkins,²

² *Jour. Am. Chem. Soc.*, July, 1911.



Periodic Table of Atomic Weights with Valence.

which puts specific volume in the place of atomic volume (at. w./sp. gr.), thus emphasizing the more important part of the atomic volume graph: the compressibility graph of T. W. Richards, changed to apply only to solids; and the graph of the fusion of the halogen compounds of Thomas Carnelley. These graphs are changed in size only to match the helix; and the exact agreement of the four large curves with the double octaves, the two small ones with the octaves, and the exact space left for the half octave is a strong confirmation of the naturalness of the helix. Further I have been able, by a very reasonable extrapolation, to prolong backward the atomic volume graph to match exactly the half and quarter octaves, and to show that its natural

culminations are in the inert gases and not in the alkalis.

The law of longitudinal condensation is especially interesting. It is only in the middle or K circle (the one containing potassium and iron) that density and valence agree from 1 in K to 8 in Fe, so that density divided by valence (D/V) equals unity. In earlier circles D/V is less than 1, so that less matter is condensed into the atom that satisfies a given amount of H, and in later circles D/V is greater than unity and more matter is condensed into the corresponding atom. Thus the densities of the iron, rubidium, samarium and platinum triads are as 8:12:16:22 + or $D/V = 1:1\frac{1}{2}:2:3$, and, if we omit gases and poorly determined elements,

															E																			
						Nu		PrF	Cn	H	Nu																							
						C	N	O	F	He	Li	Gl	B	C																				
						Si	P	S	Cl	Ne	Na	Mg	Al	Si																				
Fe	Ni	Co	Cu	Zn	Ga	Ge	As	Se	Br	Ar	K	Ca	Sc	Ti	V	Cr	Mn	Fe	Ni	Co														
Rh	Ru	Pd	Ag	Cd	In	Sn	Sb	Te	I	Cr	Rb	Sr	Y	Zr	Cb	Mo	—	Rh	Ru	Pd														
Sm	En	Gd	Tb	Ha	Er	Tm	Yb	Lu	—	Xe	Cs	Ba	La	Ce	Pr	Na	—	Sm	Eu	Gd														
Os	Ir	Pt	Au	Hg	Tl	Pb	Bi	Po	ABC	—	—	Tu	—	—	Ta	W	—	Os	Ir	Pt														
—	—	—	—	—	—	—	—	—	—	RaEm	Thx	Ru	Io	Th	Ra	Ur	—	—	—	—	—													
															Ur																			

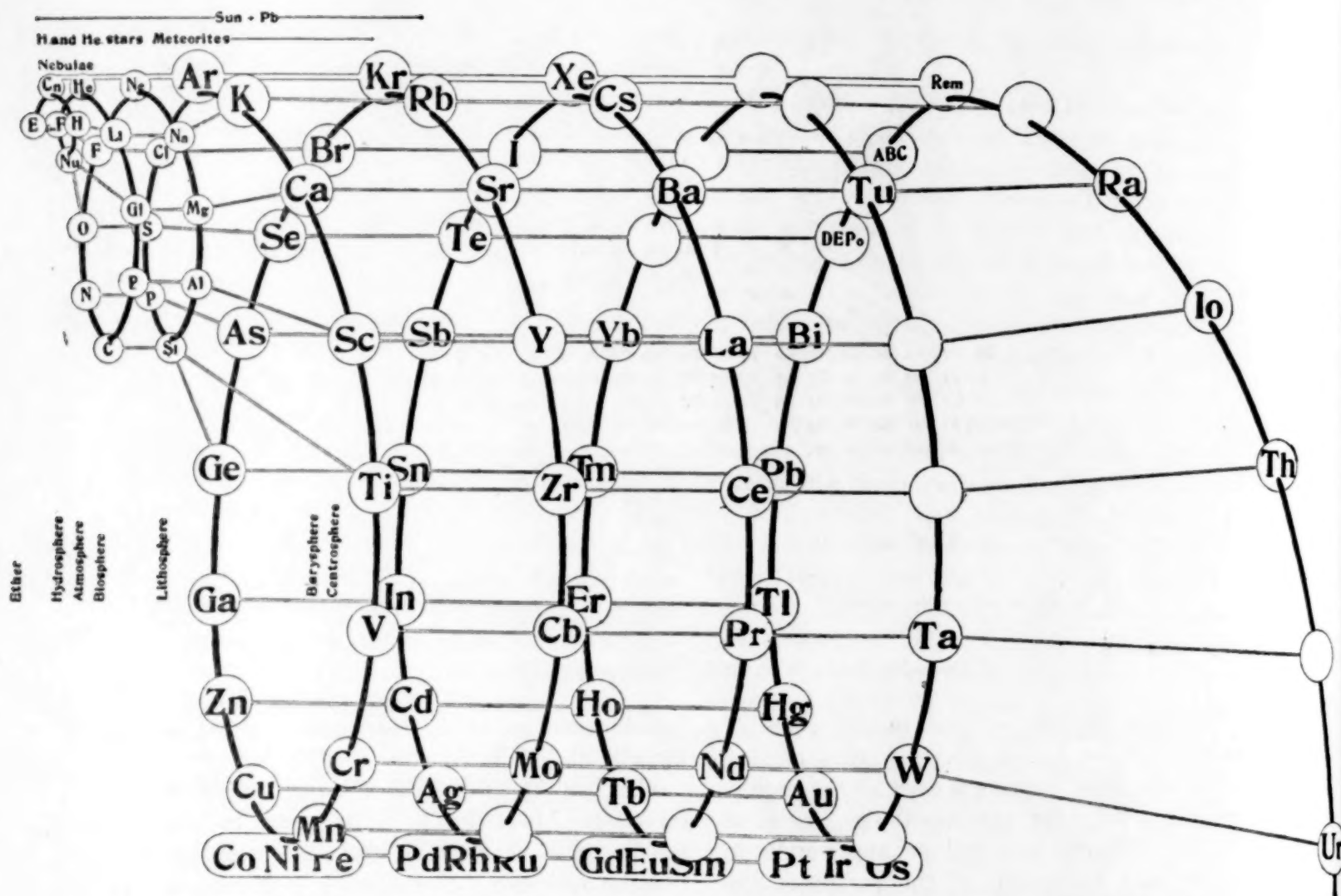


FIG. 1. A side view of the helix drawn to scale. The four graphs below have been changed, the alkalis and match closely the four double octaves, the two octaves and the half octave. The

the same ratio is true of the circles of which these triads are a part. So, omitting gases and the abnormal glucinum, we have for the carbon octave .52 and for the silicon octave .80. Thus we get, without forcing, the following average values for D/V for each of the seven circles

E	Nu	C	Si	Fe	Ru	Se	Pt
(0)	(1/3)	1/2	2/3	1	3/2	2	3.

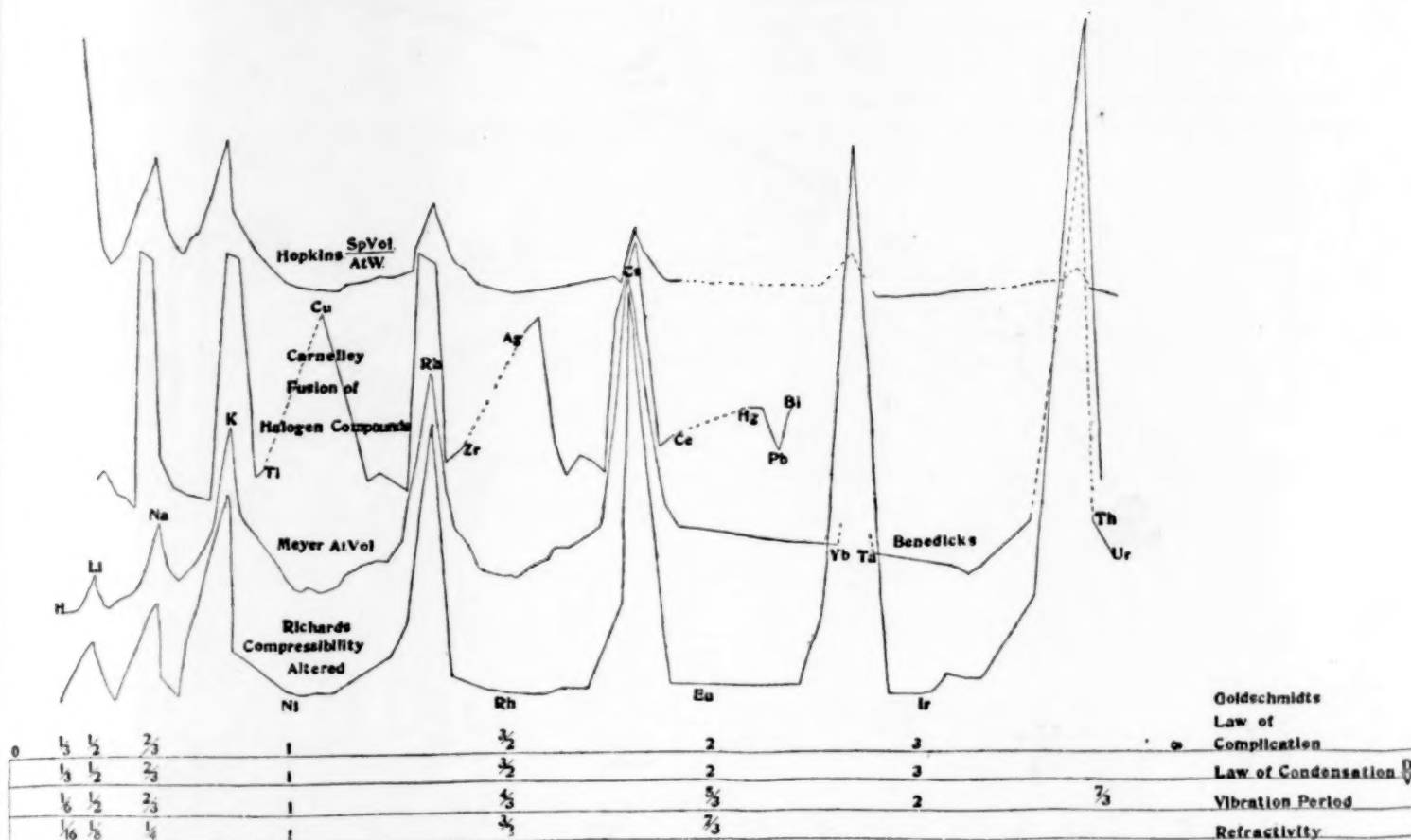
This agrees with Victor Goldschmidt's law of complication³ which shows that the law of increasing longitudinal condensation in the elements deduced above is the same as the law of the octave in music, and the fundamental law of crystallography, and the first

³ V. Goldschmidt, "Ueber Harmonie und Complication," Berlin, 1901.

two values given above are extrapolated in accordance with Goldschmidt's law.

Instead of omitting any elements as suggested above, a different and perhaps more useful line of thought may be followed. It may be assumed that the true relations would appear only when the specific gravities were taken under common conditions, say at -273° , since the elements expand unequally and sometimes diversely with change of temperature, so that we can obtain only approximate results from specific gravities taken at ordinary temperatures. We find the law most perfectly realized at the bottom of each circle at the point of greatest condensation.

Thus C as graphite is .5, Si as quartz is



in size only, to match the diagram. It will be seen that the apices of the same point exactly to table showing the longitudinal octave relations is placed below.

.66, iron is 1, rhodium is 1.5, all exactly following the law. The next circle is not determined and osmium is $2.8 +$ instead of 3.

Moreover, all the elements in the lower part of each curve keep close to this average, but as we pass up the left side of the curves into the region of lessened density we come upon a sudden sharp divergence from the law in the sulphur and halogen series, caused by the polymerization or allotropic states of these elements. If we divide the numbers of the sulphur and the first half of the halogen series by two and bromium and iodine by three we obtain numbers closely coinciding with the law in every case thus:

	Fl	Cl	Br	I	S	Se	Te
Obtained by division	.55	.65	1	1.7	5	1.1	1.5
Required by the law	.5	.67	1	1.5	67	1	1.5

We may further strengthen the argument for this polymerization by the following tabu-

lation of the monovalent specific gravities or specific gravity of the elements in the carbon octant:

Li	Gt	B	C	N	O	Fl
			.3	.3		
			coal			
.58			.5		.5	
			graphite			
	.9	.9	.9			1.1
			diamond			

As carbon crystallizes as graphite at normal pressures we may take that as the normal form and .5 as the normal monovalent specific gravity. There is a partially known graphitic form for boron and we may divide all the numbers in the lower row by two to get approximately the specific gravity of these elements in a form analogous to graphite. With these two changes we get averages of all the elements in each circle as follows:

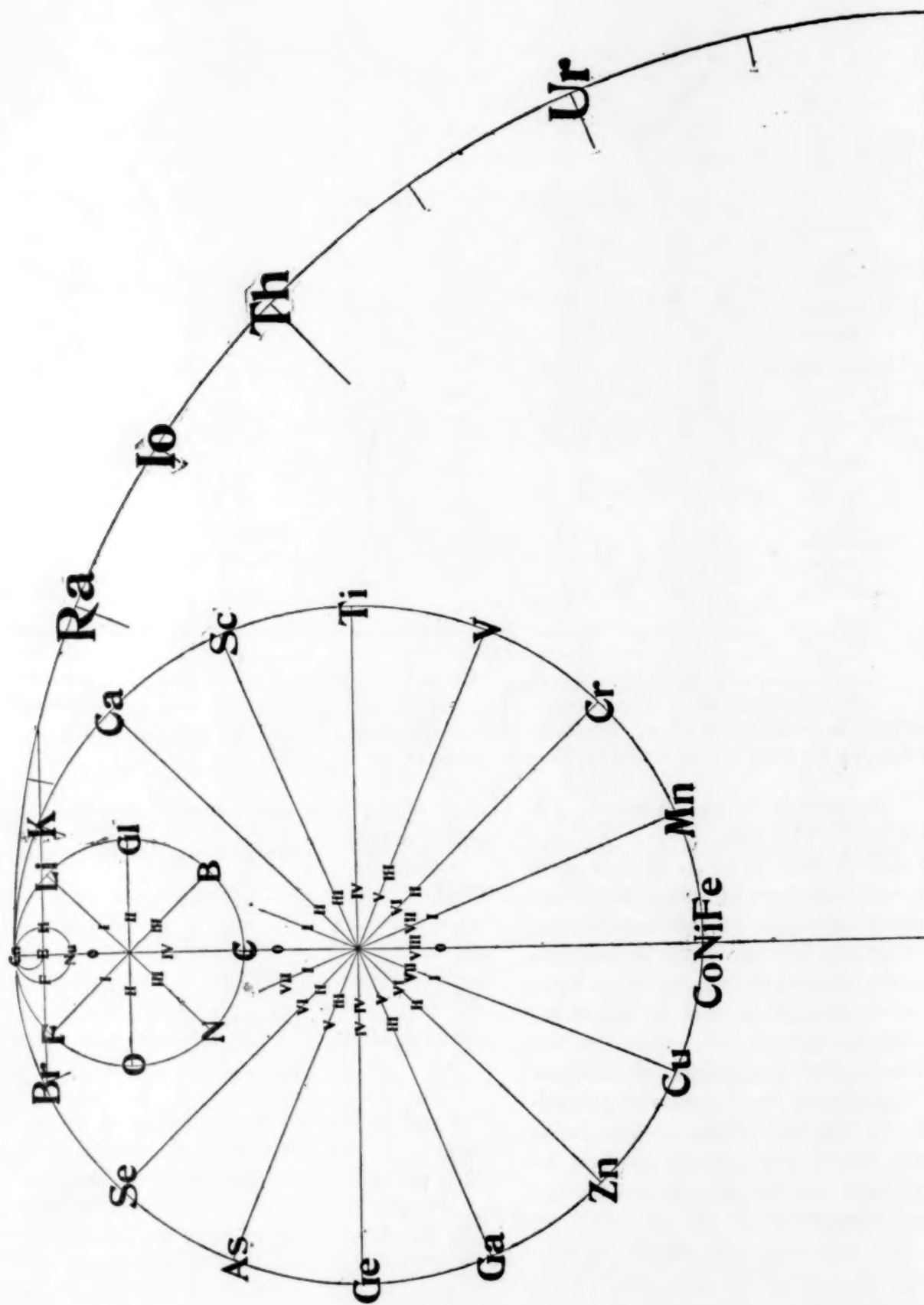


Fig. 2. An end view of the helix in true scale (except the curve *E-Cn*) showing only the first of the octaves and double octaves and symbolizing the increasing mass of the atoms. (The first F should be Proto F.)

ranged along a narrow rising band the sulphur and halogen series rise along different and much steeper lines and drop to the common band when divided as suggested above.

The transverse relations may be diametral or symmetrical to a vertical diameter (Fig. 3) equatorial or symmetrical to a horizontal diameter (Fig. 4), and ecliptic or symmetrical to an oblique diameter (Fig. 5).

II. *Diametral Relations. Fig. 3*

A vertical diameter representing a vertical plane bisecting the helix divides the elements into two groups, which are for many relations the counterparts of each other, some exactly and some approximately. Each of these relations is placed on a separate ring—an arrow indicating the direction of increase, and a cross the points of change. It is curious that some of these relations are symmetrical to a line a little to the right of the diameter and passing between C and Si, and others to a line passing to the left of the diameter and to the right of C and Si. The only complex relations are those of the Mendeléeff series and the magnetic relations, which would seem more simple if the two octants were drawn as a single double octant. The most inexplicable of all these relations is expressed in the outer circle. As we pass down the right-hand curves an addition of 1, 2, 3 and 4 units (= H atoms) successively produces the same unit increase in valence and density and thus there is great condensation; going up on the left with the same increasing addition and thus with still increasing mass, there is lessening valence and density, and this contrast is repeated seven times.

III. *Equatorial Relations. Fig. 4*

The horizontal diameter makes very simple relationships, most of which are combined with the diametral relations in quadrantal arrangement. The valence may be a simple diametral relation or may increase to 4 and then decrease as an equatorial relation. The electro-potential relations are equatorial in so far as they are all strong above the horizontal line

and weak below; quadrantal as far as the sign is concerned.

IV. *The Ecliptic Relations. Fig. 5*

The most important ecliptic relation is fusibility. On the one side are the high fusing elements, reaching a maximum fusing point at the end of an axis at right angles to the ecliptic. On the other side are the low fusing or volatile elements, reaching the maximum volatility at the end of the vertical axis. At one end of the ecliptic is the liquid mercury and at the other the luminous radium, while all the elements of the central most volatile quadrant, cut out of the volatile semi-circle by the equator and meridian, are absent from the sun, except these at the center, oxygen and nitrogen, the elements of the air.

Relations of Partial and Complex Symmetry. Fig. 6

The gas and rock areas show an antithesis which is only approximately an ecliptic relation. The gas area points upward and forward with the motion of the helix like a flame, and the rock area points downward and backward toward the center of the earth. The way the rocks and meteorites find orderly arrangement on this area from the light alkaline to the heavy ferric groups is very suggestive. The rock area typifies the increasing stability of the downward curves of increasing valence, density, and condensation. The gas area is its striking antithesis.

Attention may be called to the quadrilateral of life, CHON, in the center of the figure, surrounded by the elements on which life subordinately depends. These last are almost the only unsymmetrical relations.

A remarkable result is reached (in figures not reproduced here) by placing the elements on the helix in their true positions as determined by the real differences between successive elements rather than by the average differences for each type of circle. It is found that the agreement with the ideal position is much less perfect than might have been expected, and curious and unexpected symmetries come to light. It is further found that

the elements can be nearly as well placed on the curve by using the successive differences

other, many elements in the first being as much too advanced as those in the other are

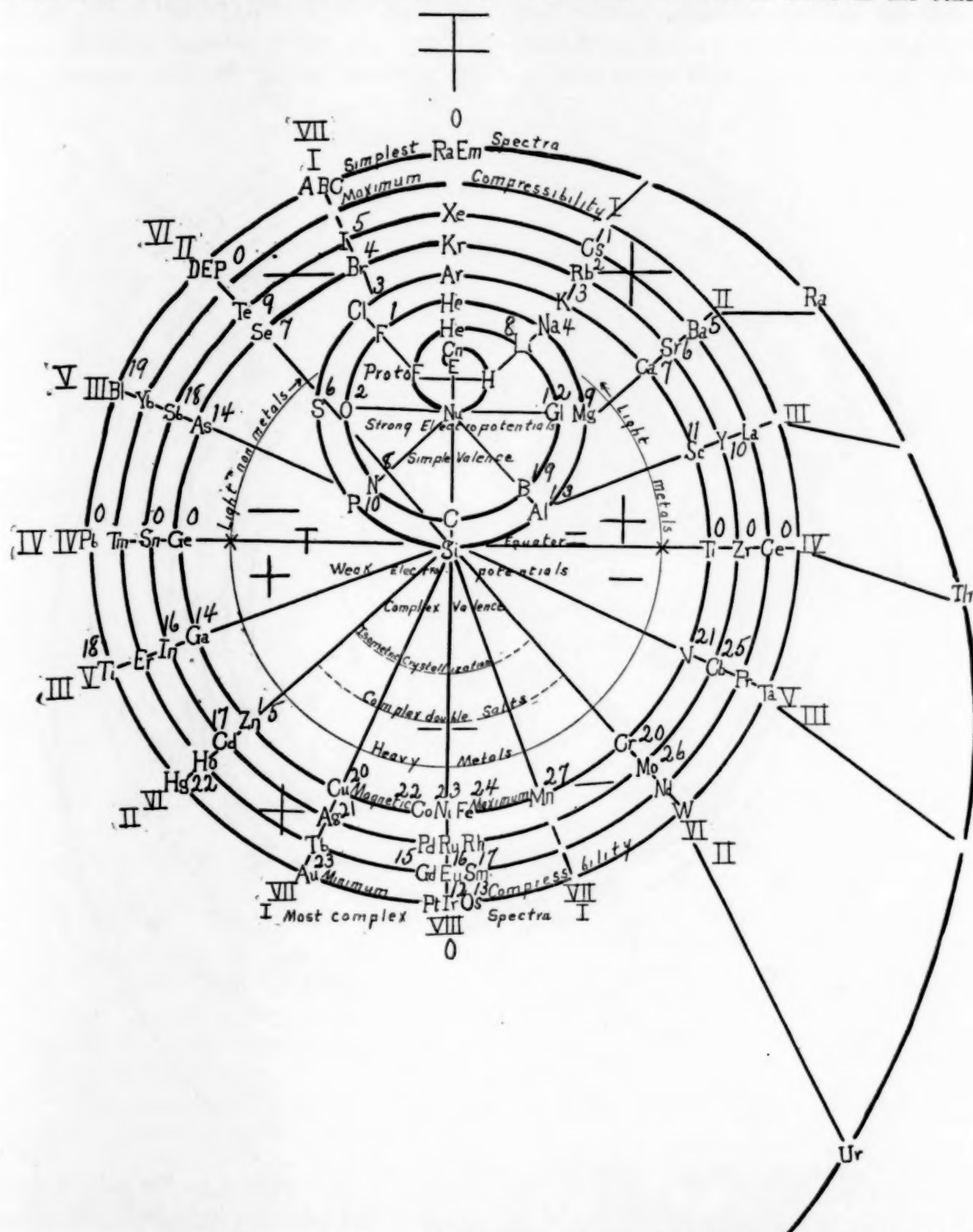


FIG. 4. Equatorial and Quadrantal Relations. Values of the electropotentials of the elements in detail. The printed numbers represent the order of these values, beginning with the highest. All the horizontal expressions refer to all the elements of the hemisphere in which they are placed.

of the densities, and that the inequalities of the two curves largely counterbalance each

retarded. So that a table made by taking the average position indicated by the atomic

3. *Evolution and Devolution*

The helix is a working model and it suggests that there is only one enduring stable

As the vix-generatrix passed over each unit space in the first circle the conditions favored the formation of a new element. In this cir-

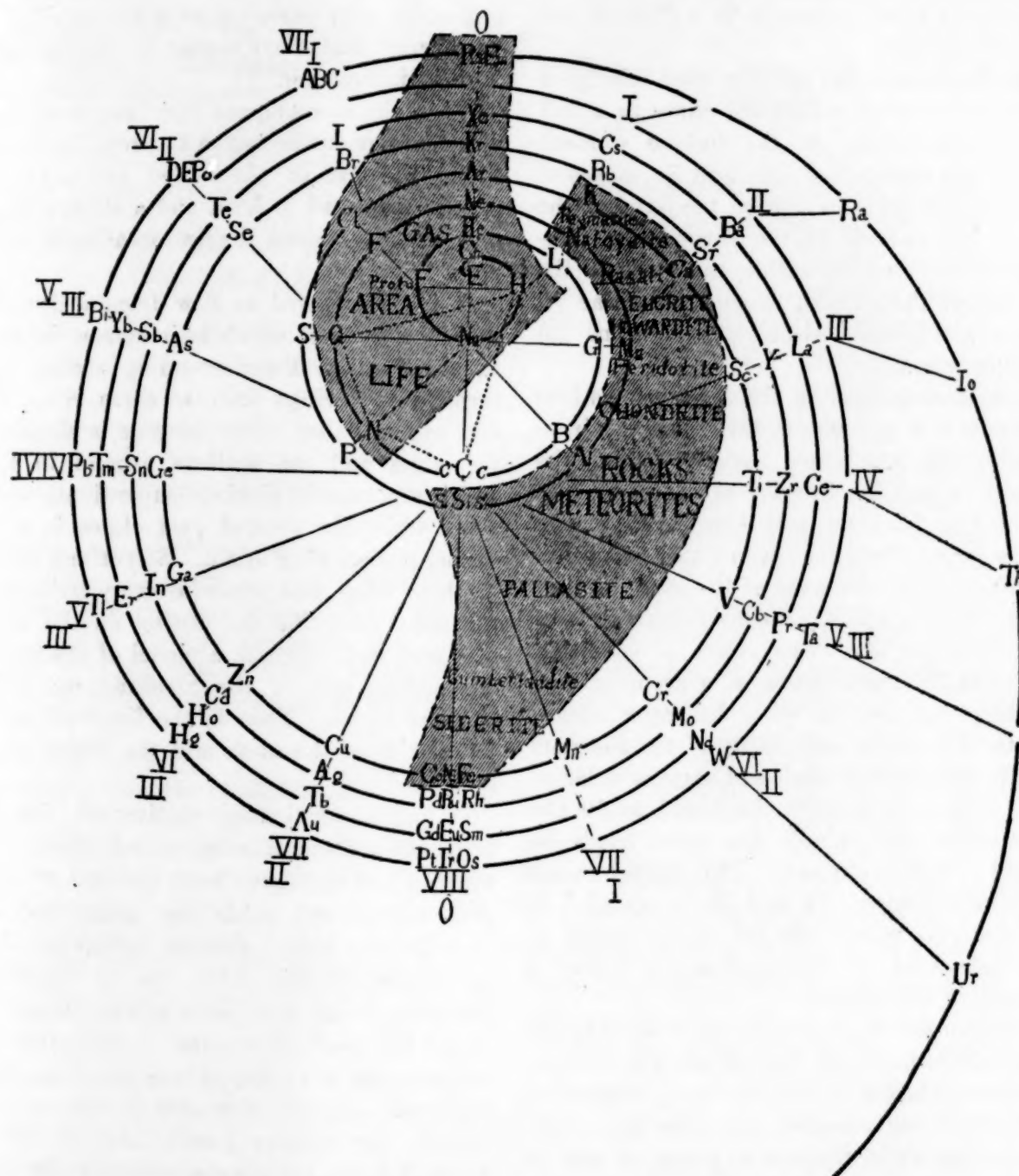


FIG. 6. Allied relations of partial or complex symmetry which do not come into the preceding categories. They are described in the section with which they are most nearly related.

assemblage of corpuscles between das Urstoff and hydrogen with its thousand corpuscles, and if that element be coronium, it has endured only under the exceptional conditions of the corona.

cle are the simplest forms of matter from hydrogen to helium. They appear abundantly in the nebulæ; existing and perhaps formed, near the absolute zero, where no heat vibrations could yet be predicated and where unex-

plained electrical activities could cause the light of the nebulae. The helix prophesies the discovery of only one new element of low atomic weight, proto fluorine, and an intensified fluorine would naturally be a difficult element to capture.

On the completion of the first circle, a double momentum swelled the curve to a full octave, culminating in the unique element carbon, the element of life, and in a second octave culminating in silicon, the basis of the rocks. In contrast to the corpuscular temperature or vibration within the atom in the elements of the nebulae, normal molecular vibration and normal electrical conditions became prevalent.

On passing potassium the treble momentum culminated in a double octave with the iron-nickel-cobalt group of the hot stars, the meteorites and the interior of the earth (see top of Fig. 1). One may surmise that most of the heavy elements beyond this group are unimportant in the mass of the earth as they are in the meteorites. A considerable molecular radiation may have been a condition precedent for the addition of a group of corpuscles equivalent to three hydrogen atoms, and the increasing complexity of the elements is indicated by their high and varying valence. Will three new elements be found to fill the gaps below Mn or will the outer triads be robbed for this purpose. The almost empty semicircle between *Yb* and *Ta* is tantalizing, but the helix would rule out the gathering of the "elements of the rare earths" into a group like the triads.

The sun carries the evolution a step further and it is conceivable that all the elements of the lowest-fusing quadrant have evaporated from it. Lead alone of the very heavy elements appears in the sun as a sort of calx or caput mortuum, suggesting that evaporation into helium has reached its limit there. It is remarkable that at the farthest place from this volatile quadrant, at the outermost curve of the high-fusing semi-circle, the radium elements should be evaporating with explosive heat and light-giving violence over all the surface of the earth: should exist in the outer

layers of the earth only, and should be brought up by the light and highly acid pegmatite, whose quartz grains have been formed below 800°.* Becker's suggestion of the genesis of pegmatite with uranium as a potentializer of the energy that must escape in its formation is most interesting.†

When the assemblages had become so complex that devolution began to prevail over evolution the size of the added group of corpuscles increased to four, and a short advance only was made along the first quadruple-octave ring.

If ink is allowed to flow from a dropping tube, the point of which is immersed in water in a tall glass, the descending stream soon forms vortex rings and, as these rings sink and expand, they often become scalloped at the edges and the scallops expand in lobes which separate in distinct subordinate vortex rings while the central part closes in as an almost perfect ring again. Sometimes several of these lobes will separate symmetrically at the same time like the fluting on the border of a rose leaf. This is a model of the radium emanations and of the breaking up of the complex atoms. Thus is the limit set to the complexity of the atom and the length of the helix.

It has a mystical attractiveness that the helix has a shape like the spiral nebula from which all things have been thought to originate, and toward which they again tend, and that the tetrahedral element carbon stands in the center of this helix and by its shapes (ranging from the tetrahedron which includes the smallest amount of matter in unit surface almost to the sphere which contains the largest amount of matter in unit surface) typifies the infinite possibilities of life of which it is the vehicle and embodies the tetrahedral form which the earth has repeatedly if imperfectly assumed.‡ B. K. EMERSON

* Day and Shepard, *Am. Jour. Sc.*, Vol. XX., p. 276, 1906.

† G. F. Becker, "Radioactivity and Cosmogony," *Bull. Geo. Soc.*, Vol. 19, p. 143.

‡ T. Arldt, "Die Entwicklung der Erde," p. 506, 1907.